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THE RÔLE OF GENETICS IN ETIOLOGICAL PATHOLOGY

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. . . . It is only in disease that we tremble at our own incredible complexity and that harmony becomes a problem . . . . the level of mechanism is so deep that it is better revealed in cataclysms, miracles and diseases than in the placid course of superficial experience. . . . .—*Santayana, The Realm of Matter* (*158*).

In so far as causation is not sheer magic imputed by laying a superstitious emphasis on those phases which interest me most in the flux of things, causation is the order of generation in nature. . . . .—*Santayana, Scepticism and Animal Faith* (*156*).

But because they [diseases] are frequent and develop among animals as well as plants in the same manner, we say that they happen according to nature. . . . .

The beginnings of the diseases in plants as well as in animals are to be found either in themselves or outside. . . . .

All the disturbances . . . . affect most seriously the weak plants, for they are less able to withstand any excess. . . . .—*Theophrastus, On the Causes of Plants, Book V, Chap. 8* (*171*). (*Translated for the author by M. S. Costas, through the courtesy of Professor Paul Shorey.*)

THE field of genetics has contributed an imposing body of experimental data and of concepts, definitions and propositions which are of significance to the science of pathology in its theoretical, empirical, and applied phases. Because abnormal as well as pathological or pathic characteristics are easily recognized, and readily followed from generation to generation in case they are not fatal, they early attracted the geneticist and have provided him with rich and rewarding experimental material. Consequently an appreciable part of the data, concepts, and propositions of genetics is based upon analysis of material which pertains to pathology. This is particularly true because most of the random changes in the genetic constitution of organisms, whether spontaneous or experimentally induced, are deleterious for the organism. In fact the geneticist experimentally creates an extensive array of pathic material which is of interest because it is specially suited to throw light on the rôle of developmental processes in the genesis of diseases and other pathic events. It seems inevitable that a study of such material will enrich both morphological and physiological pathology. In due time the pathology of the gene and of other constituents of the genetic constitutions of organisms probably will be recognized as genic and

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genetic pathology, just as the pathologies of higher levels of biological organization are now recognized as cellular and as anatomical pathology.

The experimental data, concepts, and propositions of genetics are especially significant for that phase of pathology which concerns itself with the causal analysis of diseases and other pathic events, and attempts to systematize our knowledge relative to their causation, i.e., for etiology or etiological pathology. By genetical analysis etiological pathology is able to gain an insight into the rôle of hitherto elusive and intangible internal factors of the organism in the genesis of health and disease.

I propose to present evidence arising out of genetic analysis of plants which demonstrates that the genetic constitution of a pathic organism plays a rôle as a pathogenic factor in practically every pathic event or part event. I also propose to show that genetic analysis, especially the phase that deals with the physiology of development, has strongly indicated that other internal factors, such as the past history of the organization in which the pathic event occurs, as well as correlative influences within this organization, play rôles in pathic events and hence must be considered potential pathogenic factors. Finally, I propose to show that these internal factors, together with external factors, including the influence of parasites, may be integrated into a causal or an etiological scheme or formula, which does not ignore the fact that pathic events, like other biological events, usually arise out of a *causal complex* rather than from a *single cause*. While the discussion is based on material drawn almost entirely from the fields of plant pathology and plant genetics, the analysis and synthesis attempted here are applicable to animal pathology and show that etiological pathology may serve as a basis for a comparative pathology which includes the entire fields of animal and plant pathology. The treatment does not pretend to do justice even to all pertinent plant material. While it may seem that an undue emphasis has been placed on the corn plant, it should be borne in mind that this is better analyzed genetically than any other plant.

The first part of the discussion is devoted to an analysis of the present status of pathology, especially plant pathology, so far as etiological concepts and propositions are concerned; the second, to an analysis of the fundamental concepts, propositions, and definitions of pathology; and the third, to a fusion of the concepts and propositions of pathology and of genetics, with a brief statement of the historical background of the separation which exists at present.

ETIOLOGY IN CONTEMPORARY PLANT PATHOLOGY

Correns (50) in 1920 pointed out the significance to comparative pathology of the data obtained by genetic analysis of all types of pathic characteristics of plants. This paper apparently has not affected the current of phytopathological thought and experimentation to any great extent, and the full implications of the data and propositions of genetics for etiological plant pathology have not yet been fully developed in any treatise on plant pathology. The most adequate treatment of this subject is found in the excellent volume by Morstatt *Einführung in die Pflanzenpathologie* (130). Morstatt presents the proposition that infectious diseases arise out of a complex of factors comprising the parasite, non-living external factors, and the varietally conditioned susceptibility of the host. Unfortunately, his treatment of etiology

concerns itself almost exclusively with the rôle of non-living external factors and of parasites, and the rôle of internal factors is touched only in passing. The rôle of hereditary transmission of diseases is briefly mentioned in connection with non-infectious chloroses and torsion. It is unfortunate also that his treatment of the rôle of the constitution and disposition of the affected organism in pathic events was not made part of his discussion of etiological pathology.

Usually, in American and English phytopathological literature the discussion of the etiology of a disease concerns itself only with one factor, which is singled out and designated as "*the cause.*" In infectious diseases this is the parasite, while in the so-called non-parasitic diseases it is the obvious external factor. The rôle of genetic factors is treated under the concept "susceptibility," and that of non-living external factors under the concept "predisposing factors." In the German phytopathological literature the concept of an etiological complex is formally recognized by some by designation of the parasite or of the releasing non-living external factor as "*Erreger,*" i.e., "incitant" or "excitant" rather than as "*the cause.*"

OVEREMPHASIS OF RÔLE OF PARASITES

Plant pathologists have in the main busied themselves with diseases incited by parasites, especially by bacteria and fungi, because of the great number and great economic importance of these diseases; and because of the mycological stamp put upon phytopathology during its formative period from 1850 to 1870, and the resultant delineation of the ideology and methodology of plant pathology in the organization of federal and state research, teaching and service institutions. They have amassed an imposing array of empirical data bearing upon the phenomena of parasitism. One line of this research has concerned itself with biochemical and biophysical studies of infective agents and of the mechanisms of resistance to infection. Considerable attention, however, is also given to physiological, biochemical, and biophysical studies of the changes induced by the parasite and of the effects of the host on the parasite. Another line of research has studied the relation of various parasites and hosts to non-living environmental factors. Quantitative studies of the relation of these factors to the biology of parasites advanced with commercial development of incubator and refrigeration equipment which made possible, notably in the United States Department of Agriculture, and later elsewhere, a study of the relation of temperature to development of fungi and bacteria which incite decay in storage organs of plants. This field of study has developed enormously, following the invention and application, first by Hottes (89), Jones (100, 101), Johnson (97), and later by many others, of apparatus whereby soil and air factors of growing plants may be controlled. It has yielded an interesting and significant array of quantitative data relative to the reaction of parasites to various non-living environmental factors, such as the temperature and the moisture content of the air and soil, as well as of light, and the pH of the soil. In addition it has experimentally verified the thesis of Hallier (84), Sorauer (166), Ward (183), and others that environmental factors play a conditioning rôle in infectious diseases through their influence on the host plant, and disproved the contention of the germists, who attempted to maintain that the parasite is *the sole cause* of infections or the so-called parasitic diseases. A third line of research

which is even more ecological in nature is concerned with biologic specialization of parasites with reference to their hosts. This has been carried on most intensively for the smut and rust fungi both in Europe and in America, and the results have been summarized by Reed (144) and Stakman (167).

Fischer and Gäumann (78) in the excellent treatise *Biologie der pflanzenbewohnenden parasitischen Pilze* have recently (1929) organized the vast body of data bearing upon the ecology of fungi parasitic on plants, with detailed consideration of the rôle of genetic factors of both host and parasite in the establishment of parasitic relations. During the first decade following rediscovery of Mendel's Laws (126), Biffen (22, 23, 25) demonstrated inheritance of susceptibility and resistance to rust and mildew for cereals, and Norton (132) and Orton (138) demonstrated the inheritance of susceptibility and resistance to fungus invasion for tomato and melon. From these beginnings an amazingly large body of data accumulated, which found extensive practical application in disease control, but which lacked theoretical treatment and organization.

The genetic and non-genetic factors of parasites which condition their invasiveness and virulence are also considered by Fischer and Gäumann (78). A great body of work with bacteria and fungi had indicated that these characteristics, while in part conditioned by external factors, also vary in different strains of the same species and might be genetically conditioned. Recently, following Kniep's (105) successful hybridization of strains of the smut fungus *Ustilago violacea*, Goldschmidt (81) completed the proof and demonstrated that specialization of biological species of *Ustilago violacea*, in other words, specific invasiveness and virulence of strains of the same species as measured by their behavior toward different hosts, is in part genotypically conditioned.

Unfortunately there is no utilization in plant pathological literature comparable to that made by Fischer and Gäumann (78) of genetic data and concepts bearing upon non-infectious diseases incited either by non-living external or by internal factors. Since 1860 the main current of plant pathology has not concerned itself with anomalies of form or malformations, excepting those incited by parasites. As a consequence the etiological concepts and propositions of plant pathology have not been influenced by the work of Sachs, Pfeffer and Goebel and others who have fostered the proposition that internal factors condition development, nor by those studies of geneticists which demonstrate that genetic factors condition pathic malformations. By the time De Vries (179, 180) and Correns (40) had reported that teratological characteristics arise by mutation and are inherited in Mendelian fashion, that Correns (38), Bidgood (21), Baur (12), and Hurst (90, 91) had shown that fatal and less severe chlorosis and albinism are hereditarily conditioned, and Biffen (24) and Salaman (155) and Tschermak (173) had shown that sterility and winter hardiness are Mendelian characters, the etiological concepts and propositions of plant pathology through the influence of De Bary (8) and Kühn (106) had crystallized under the spell of a simple version of the germ theory.

ETIOLOGY IN CONTEMPORARY HUMAN PATHOLOGY

In the main the treatises on general human pathology, of which there are many more than on general plant pathology, formally recognize heredity as a factor in pathogenesis. Unfortunately, however, here too, as in plant pathologi-

cal literature, one type of factor is often sharply contrasted with all the other causes or conditions, and the proposition still is widely current that if a characteristic or disease is hereditary, factors other than genetic ones do not play a rôle in its genesis, and that if it is non-hereditary, genetic factors can not play a rôle. However, there is general recognition that the internal factors or the constitution of an organism, as well as environmental factors, enter into the etiological complex of all types of disease. In fact, under the stimulus of genetics there has been a revival of emphasis of the constitution of the human body as a pathogenic factor and a new literature has developed notably by Siemens (165), and J. Bauer (10, 11). In general, in human pathological literature the infective agent in infectious diseases is designated as *excitant* or *incitant* and not as "*the cause.*"

A very satisfactory presentation of etiology, including the rôle of genetic and non-genetic factors, has been given by Rössle (5) in his contribution to Aschoff's *Handbuch der pathologischen Anatomie*. Oertel (137) also formally recognizes heredity as a pathogenic factor.

The rôle of genic factors in the genesis of diseases and anomalies of man is well presented by Lenz (18) in *Menschliche Erblichkeitslehre*. Jennings (96) in *The Biological Basis of Human Nature* has given the most comprehensive and adequate discussion of the rôle of both genetic and non-genetic factors in the genesis of disease, and shows their interrelatedness, even though problems of pathology are not the main theme of the book.

PATHOLOGY AS BIOLOGICAL SCIENCE

In view of the fact that arbitrary limitation of the concept of disease and of the field of plant pathology has tended to prevent adequate consideration of the principles of etiological pathology, I shall devote what may seem to some experimentalists, and to those interested in practical control problems only, an inordinate amount of space to discussion of the concepts, propositions and definitions of pathology and of genetics. I feel justified in this because I am convinced that etiological data will not be adequately integrated so long as the fundamental biological principles of theoretical pathology are ignored. Furthermore, pathology will not develop as a theoretical science so long as it does not heed the reflection of Bernard that "for every science it is impossible to proceed from a known fact to an unknown fact without the aid of an abstract idea or theory."

I consider pathology a branch of biological science which is not merely oriented toward crop protection or medicine. It is a branch of biological science in the same sense as are physiology, genetics, and mycology, and as such, it has theoretical, as well as applied aspects. In its theoretical and general phases it is like any other science, a *system* of knowledge, or, in the words of Poincaré (142) it is "before all a classification, a manner of bringing together facts which appearances separate though they were bound together by natural and hidden kinship." Through neglect of its theoretical aspects and preoccupation with its empirical and practical phases, theory and systematization have lagged behind practise, and plant pathology has failed to build a formally expressed system of relations.

"Sciences," wrote Santayana (159), "are dreams abstracted, controlled, measured and rendered scrupulously proportional to their occasions. Knowledge accordingly always remains a part of the imagination in its terms and in its seat; yet by virtue of its origin and intent it becomes a memorial and guide to the

fortunes of man in nature." Science undoubtedly had its origin in the practical arts, and if it is to remain humanly effective it must maintain its connection somewhere with practical problems. These are the original occasions of the dreams of science. If, however, these occasions are to be narrowly and immediately conceived, no matter how much diligence and discipline are brought to bear on them, the result will miss the deep seated and fruitful relations, so that instead of a science there merely accumulates a disconnected series of recipes.

I do not mean to depreciate the practical things plant pathology has done nor counsel that it cease dealing with them. I counsel rather that it cultivate more intensively its theoretical aspects. This attitude is not based on a lack of interest or experience in practical phases of plant pathology. For fourteen years my time and energy were primarily directed toward practical pathological problems. During the last seven years of this time my efforts were devoted to incorporating a biological point of view into the newly developing organization and activities of the Fresh Fruit and Vegetable Inspection Service of the Bureau of Agricultural Economics of the United States Department of Agriculture (118, 119, 120, 143), whose influence and activities now affect the entire fruit and vegetable industry and have aided in making fresh fruits and vegetables available throughout the year in all parts of the United States.

The history of science during Roman times shows that a science pointed to practical applications alone soon ceases to be fertile and active. A science soon becomes sterile if it lacks a spirit of disinterested inquiry, a pursuit of the subject without reference to its application to particular instances. Even if one could feel that the full fruits of the application of science were shared by all rather than by a few in socially and economically strategic positions, or that they were finally exploited to develop a happier and richer social body, there would still remain the innate motive and aim of science, namely, satisfaction of curiosity about nature and the pleasure which comes from seeing relations simply and beautifully revealed.

There is more to plant pathology than the immediately practical. Even if it were possible so to control the factors which decrease crop production and destroy the harvested crops, that enough plant materials were produced for all needs with a minimum of labor and investment, pathology would still retain its real reason for existence, namely, the formulation of precise, simple relations which pertain to pathic events in plants. For, as expressed by Poincaré (142),

> The scientist does not study nature because it is useful: he studies it because he delights in it, because it is beautiful. If nature were not beautiful, it would not be worth knowing, and if nature were not worth knowing, life would not be worth living. Intellectual beauty is sufficient unto itself, and it is for its sake, more perhaps than for the future good of humanity, the scientist devotes himself to long and difficult labors. We see too that the longing for the beautiful leads us to the same choice as the longing for the useful. And so it is that this economy of thought, this economy of effort, which is, according to Mach, the constant tendency in science, is at the same time a source of beauty and a practical advantage.

The history of any science shows that the meaning of its concepts, propositions and definitions consists of their possible deductions or consequences. Ideas or concepts and propositions are patterns for action in the experimental sciences, and determine not only the questions asked, but also the answers anticipated, and, unless one is always on guard, the answers

obtained. Despite the inherent difficulties, attempts at precise definition are essential in biology as in other sciences, for as Poincaré (142) remarked in reference to the evolution of mathematics, "It was not slow in being noticed that rigor could not be introduced into the reasoning unless first made to enter into definitions."

II

What are the fundamental concepts and definitions of plant pathology? Beginning with Theophrastus (170, 171), various opposite concepts such as healthy and diseased, normal and abnormal or anomalous, physiological and pathological, have been current in phytopathological literature. Do these terms have an objective basis in the characteristics of plants?

HEALTHY AND PATHIC

Plants, like other living things, are organizations characterized by processes which occur in response to stimuli and not only maintain the organization derived from the parent or parents in approximate dynamic equilibrium (maintenance processes or functions), but also manifest themselves in growth, development, and reproduction (growth processes or functions). In development the organization differentiates and elaborates so that it is characterized by spatial differences of its various parts and processes at any instant as well as by temporal differences in the form of the rhythmical and serial changes which constitute its life cycle (Woodger, 196).

It is a further characteristic of a living system that from the very beginning, under guidance of its inherited organization, its structures and activities in interplay with their respective environments tend to develop in such harmonious integration, coördination, and automatic regulation that the whole is able to respond to the stimuli of its environment in such manner that it tends to maintain itself with minimum change. It responds or reacts appropriately as measured by persistence with progressive, rhythmical and serial change, and consequently is said to be fitted, adjusted or adapted. While this adjustment is possible under diverse conditions it occurs only within characteristic ranges of easy and absolute tolerance. These tolerances are such that appropriate responses occur, not only to the constant and recurrent features of the various environments, but also to those which are unusual or entirely new. This dual aspect of adaptation may be designated as adaptive capacity. In general, the greater its range, the greater the chances of survival of the organization in question.

When a growing or mature organization reacts so appropriately that it is fully adapted and capable of readily maintaining itself and its kind, it is said to be healthy. The abstract term health has not only a present but also a prospective implication. It designates a state of full functional competency, a state in which all structures and activities are so harmoniously integrated and regulated that easy maintenance of the whole organization is continuously insured. A healthy plant appears thrifty and vigorous.

Sooner or later, in the course of its life cycle, events may and usually do occur within an organism with more or less frequency during which its reactions to internal and external stimuli exceed the limits of easy tolerance or even of its absolute adaptive capacity. These events are characterized either by an initial lack, or by a disturbance with temporary or permanent loss, of that harmonious inte-

gration and coördination of structures and activities by virtue of which a healthy organization is presently and prospectively adapted to its environment. If not forced too far it eventually establishes new adjustments after a period of non-adaptive reaction; if its absolute range of tolerance is exceeded it disintegrates and dies.

Happenings or changes in the various levels of organization of living systems which do not fall within their range of easy tolerance or adaptiveness, I designate as *pathic events*; the levels of organization in which they occur, as a pathic gene, cell, tissue, organ, individual or species, as the case may be; and the factors or events which condition them, as *pathogenic factors*. Pathic events, as we recognize them, are isolates from the larger event, the life cycle of the organism, and usually are complex and extended in time. For purposes of analysis they may be cut into smaller isolates or part-events, with the result that the part-event of one moment appears as the pathogenic complex of the part-event of the next instant. If the pathic event is characterized by progressive changes during successive intervals it may be designated as a pathic process, while if its characterization remains essentially the same during long intervals, it may be designated as a pathic state. If one focuses attention on the persistent aspects of a pathic event, it appears as a state, whereas if attention is focused on the progressive changes, it appears as a process (Figs. 1, 2, 3).

No objective basis has yet been devised for determining precisely where the healthy ends and the pathic begins. Use of the concepts healthy and pathic involves the principle of polarity, "the principle," as Cohen (36) states, "that opposite categories like identity and difference, rest and motion, individuality and universality, etc., must always be kept together though never identified." It so happens that in biology as in common sense, it is worth while and convenient to distinguish between healthy and pathic. It should be remembered, however, that upon close analysis the line between the healthy and the pathic cannot be drawn any more sharply than the line between life and death.

In the realm of abstract science, (states Cohen (36)), it is as possible and worth while to distinguish life and death as to distinguish positive and negative magnetism. But it does not follow that either has any separate existence in nature; and in fact the concrete biology of organisms is unintelligible apart from the processes which lead to death. Anabolism is unintelligible apart from catabolism. Since the days of Virchow pathologists have given up the attempt to look upon the processes of disease as foreign to the processes of life. . . . . Hence though the values of life and death are most sharply antagonistic, we cannot explain the actual phenomena of biology by reference to life purposes only.

The term pathic is derived from the Greek pathos (*πάθος*) which, in the original and most general sense, means "anything that befalls anything." Later its more restricted meaning, "anything bad that befalls anyone" came into more general use. Aristotle (4) and Theophrastus (170, 171) used the term in a generic sense and considered nosos (*νόσος*), that is disease, as one kind of pathos. I prefer to use the term pathos and its derivatives in this generic sense rather than as the equivalent of our term disease, which now is often used with a restricted and technical implication like the Greek nosos, but which originally had the broad meaning of *dis-ease*. If *dis-eased* is used in this earlier sense, its meaning is essentially equivalent to pathic as I use it, and is the contrary of healthy. I use the adjective pathic in preference to pathological as a more convenient term in designating all events which constitute the subject matter of pathology, and reserve pathological as

the adjective of pathology (the system of knowledge dealing with pathic events).

NORMAL AND ABNORMAL

Pathic events and organisms frequently are designated as abnormal (abnormalities) or anomalous (anomalies). These terms have acquired several meanings and their use without further specification usually leads to endless confusion. Sometimes they are used as contraries of healthy, i.e., in a functional or physiological sense. At other times they are used to designate deviations from the usual, i.e., in a statistical biometric sense, or from the ideal, which is based on some one's judgment or wish.

This leads us to consideration of real variability, which is another pervasive characteristic of living systems. Rarely are two individuals, even of the same clone or variety, identical in all respects. This variation may be so slight that certain characteristics, combinations of characteristics, or individuals appear with sufficient regularity to be considered (for scientific purposes) the usual or the regular. They are often designated as the normal, while the unusual or irregular ones are designated as abnormal or anomalous. When used in this sense these terms have statistical and biometrical meanings, referring to frequency of occurrence, or of distribution of various possibilities of form or reaction. Sometimes the term normal is applied loosely to the most frequent classes in a range of variation of a characteristic; sometimes to its most frequent class (the mode); and sometimes to the average or mean of its classes. The terms abnormal or anomalous are applied to the less frequent classes, or to deviations from the mode, the average and the mean.

These values have been determined quantitatively for various characteristics and even for certain combinations, but in no case have they been determined for all the characteristics of one plant, either singly or in combination. Hence when the phrases "normal" or "abnormal plant" are used in the statistical sense they are merely expressions of what some one considers the usual or the ideal, or their contraries. The ideal takes on tremendous significance for an organism when it becomes the standard or norm in selection and breeding.

Such phrases as "normal health" and "abnormal disease" consequently are not necessarily tautological. Also, characteristics which are normal in the statistical sense need not be healthy (normal in the functional or physiological sense), nor need those which are abnormal in the statistical sense be pathic (abnormal in the functional sense). Thus death, which most species circumvent only by a tremendous fecundity, is the usual or normal end of multicellular individuals, but this does not mean that it is a normal event in the physiological sense. Mutations are abnormal in the statistical sense but not all mutants are pathic. It is true, however, that usually the statistically normal or ideal characteristics are also the adaptive, healthy or functionally (physiologically) normal ones, because the nonadaptive ones tend to be lost in selective elimination. There is only a tendency in this direction, however, because the environment does not exercise an absolutely rigid control. Different kinds of organisms flourish in the same environment; new ones, more successful than preceding ones, on occasion arise in the same environment; and many organisms live "dragging a long chain of idle tricks, diseases, and obsolete organs" (157).

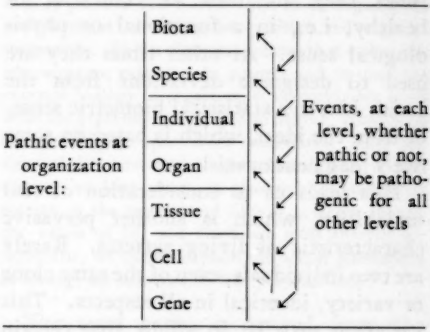
RANGE IN SEVERITY OF PATHIC EVENTS

Pathic events range in severity from those characterized by slight departure

from the range of easy tolerance, through those in which the organism or its parts are living at the limits of their absolute tolerance or adaptive capacity, to those in which such limits are exceeded, with the result that the living system dies. Unless a pathic event involves sudden destruction of the living system it is accompanied or followed by reparative processes during which the remaining organization tends to establish new adjustments. Marked deviations from the state of full adaptedness are by some (18) designated as "diseases" in contrast to the slight permanent deviations. The latter are then designated as anomalies, which is a restricted use of the term. Although slight permanent deviations are not a direct threat to persistence of the organism, they often affect functional competency so adversely that the organism is predisposed to serious pathic events. Many such anomalies therefore constitute susceptibilities or diatheses. A living system whose organization is such that occurrence of a pathic process at a definite stage of its life cycle is more probable than is characteristic for its kind, or whose organization is such that the occurrence of a pathic event is inevitable or more likely than usual for its kind in the usual or normal environment of the species, may be said to be in a pathic state. Similarly an organism which was the site of a pathic process but has recovered with a permanent lesion or injury which entails a permanently diminished adaptive capacity, may be said to be in a pathic state. Thus an acute infectious disease, such as potato or apple scab, is characterized by processes which lead to a pathic state in the form of the lesion or scab spot which endures long after the processes which produced it have ceased. The presence of the lesion, however, increases the chances for other pathic processes in the modified region.

PATHIC EVENTS AND LEVELS OF BIOLOGICAL ORGANIZATION

Pathic events occur and must be evaluated at each level of the hierarchy of organization of organisms. This is diagrammatically indicated in the following scheme:



Pathology has in the main concerned itself with the individual level of organization. However, in its attempt to explain events at this level it has extended its analysis in histological and cellular pathology through the lower levels down to the cell. In the third part of this paper it is contended that analysis should be extended to the gene level (genic pathology). In its analysis of the individual level it has also been found necessary to reach into the species level and the level of association of one species with another, the biota. Disturbances in fertilization and sterility are problems at the species level, while parasitism, depredation and disturbances incident to cultivation by man, are problems at the biotic level.

An event may be pathic at one level of organization without conditioning a pathic event at another level. Because of the low level of organization or integration

in even the highest plants, whole organs may be so pathic that they die without conditioning pathic events in the rest of the plant. This also is true for many animals of the lower phyla. On the other hand, an event which may or may not be pathic at one level may condition a pathic event at another, that is, play a pathogenic rôle. Failure to recognize these facts has led to much confusion in the use of the term pathic.

It is a well known fact that pathic events at one level of organization are involved in the healthy or physiologically normal development and functioning of another, higher level. Thus death of cells may be involved in development of tissues and tissue systems, and impairment of the full adaptive capacity of an individual in certain directions may be essential to the life of the variety or species. Thus development of cork cells, or of the ducts of the vascular system of plants, involves death of certain cells. While it is physiologically normal for these cells to die, if the organism as a whole is considered, they are the site of pathic events, from the point of view of cellular pathology. Sometimes, on the other hand, pathic events which are critical for the organization as a whole may occur in a few cells in a tissue or in an organ or organ system, and directly or indirectly result in the death of the organism.

The criteria of apathic and of pathic events are therefore relative. This is especially apparent when one considers an event with reference to both the individual and the species level of organization. Thus, lack of opportunity for reproduction is a pathic event if the persistence of the species is stressed, even though it may not decrease, but rather increase the chances of survival of the individual. On the other hand, reproduction and senescence may be pathic events so far as the individual is concerned, but adaptive for the species, because they make way for individuals that may reproduce and have greater adaptive capacity. From the biological point of view, persistence of the species is more important, and consequently it is usually taken as the criterion of adaptive capacity. In evaluating the adaptive capacity of an organism one usually does not measure this by its capacity to persist in all possible present and prospective environments but in the environments usual or normal for the species.

This brings us to a consideration of a further complication which arises when events in plants are evaluated by the standard of human needs and ideals. In cultivated plants, a very important factor, indeed the most important, in their environment and their survival, is man, with his practical demands, and often highly arbitrary and passing whims of pleasure and satisfaction. The desires and standards of man become the greatest single factor in selective elimination. In fact, in cultivation, plants are often selected and propagated which would not survive in the wild state. Those characteristics of cultivated plants by virtue of which they meet the demands of man, may therefore be considered normal, perfect, or healthy, and deviation therefrom, if one chooses, abnormal, pathic, imperfect, or merely undesirable. These considerations, of course, do not concern pathology as a pure science, whereas they are its central theme as an applied science.

TYPES OF PATHIC EVENTS

All pathic events are frequently designated by the collective term "disease," and pathology is accordingly defined as the science of disease. When the term disease is used in this sense it is used as the

contrary of health. Common usage, however, also finds it convenient to recognize different types of pathic events, and distinguish between disease, in a more restricted sense of the term, malformations, injuries, and death.

In its restricted sense the term disease is applied to those pathic events which are characterized by a sequence of changes, i.e., by processes, extending through a considerable period of time.

Malformations are variations in form of one or more organs or organ systems, or of whole organisms, which arise in the course of development and lie outside the statistical or ideal normal. Originally and strictly the term was applied only to readily recognizable deviations in external form. Now, however, the term is also applied to deviations in the physical and chemical structure as well as the resultant functions. It also is applied to deviations in the minutest levels of organization, such as the cell and the gene. Malformations may be characterized by deficient, excessive, or new development.

Slight malformations are often referred to as anomalies, abnormalities, or aberrations, while severe or striking ones are called monstrosities. If the aberrant forms also lie outside the range of easy tolerance or even of absolute adaptive capacity, their development constitutes a pathic process or disease. If they are not fatal during development they may merely force the organization beyond its range of easy tolerance, thus constituting a pathic state, or they may subsequently lead to such lack of coördination and regulation that further pathic processes result. In evaluating them, both their present and prospective effects must be considered. Since in most plants new tissues and organs arise until death, pathic events in plants are characterized by malformations to a greater extent than is the case in higher animals. Most susceptibilities or diatheses may be considered malformations. They are characterized by a lack of vigor so that the organization is unfavorably affected by stimuli to which most of its kind react adaptively.

A malformation which is very slight and not pathic at one level of organization, may condition a severe or fatal outcome in the higher levels, as will be seen when we discuss genetical problems. Thus many genes which are aberrant, abnormal, or anomalous in the statistical sense, are normal in the physiological sense in that they generally are able to maintain themselves, but pathogenic in that they condition lethal effects in cells, tissues, organs, or whole organisms.

Since injury, or disturbance of the ideal state of easy tolerance or functioning, is the essence of all pathic events, pathology may be considered to be the science dealing with injuries. The term injury, however, is used on occasion in a more restricted sense and designates pathic events which are of sudden or violent onset, such as gross injuries and wounds—and in which there is an obvious conditioning factor. The term is also applied to the enduring consequences of pathic processes, such as lesions. Injuries are often differentiated on the basis of the nature of the injurious agent and of the manner in which it is applied, i.e., whether it acts suddenly and violently, or insidiously.

A high proportion of the events considered by pathologists are reactions of living systems to injuries, and not the injuries themselves. They are the consequences of injuries, the reactions of cells, not directly injured, in the course of establishing new adjustments. This preoccupation of pathology with the consequences of pathic events is in part due to the fact that many injuries probably consist in structural changes which are transi-

tory or on a sub-visible scale, while the reactions thereto and their consequences leave more permanent and readily detectable structural changes. Consequently many events which are called diseases really are the consequence of disease. All reparative processes and structures are an illustration.

When a living organization approaches the limits of its adaptive capacity it becomes highly pathic. When internal or external factors force it beyond these limits, degeneration with a gradual or a catastropic death results. Consequently death is the final and most severe of all pathic events of an organism, in that it terminates a long biological continuity. Strictly, death is characterized by cessation of all processes that characterize the living state (111). Necrosis or local death of cells often is significant in that it directly or indirectly plays a pathogenic rôle with reference to other cells, tissues, organs, or even the entire organism, in most cases of general death.

The static descriptive phase of human pathology is so much concerned with the dead body that human pathology is often designated as necro-pathology. This is due in part to the medico-legal significance of post-mortem determination of the "causes" of death. This was the earliest reason for examining the human body during the Middle Ages, and out of it grew anatomical study and anatomical pathology, which for a long time flourished more than the former because man's native interest is in the unusual and pathic. A similar preoccupation with the dead body does not exist in plant pathology, and this difference in viewpoint is reflected in the comparatively restricted development of anatomical pathology, to which only one treatise, that by Küster (107) has been devoted. The dead plant body does not usually have the legal significance of the human corpse and consequently both agriculturist and pathologist are mainly interested in the plant corpse only in so far as it aids in a diagnosis which gives a clue to measures which will prevent a similar occurrence in other plants. Recently, with the enormous development of distribution of fresh fruits and vegetables a similar development has occurred in plant pathology in connection with the need of diagnosis of pathic events and determination of their causes for legal proceedings arising out of claims for losses of fruits and vegetables in transit and storage.

PHYSIOLOGY AND PATHOLOGY

A sharp distinction is frequently made between physiological and pathological. As experimental sciences, both physiology and pathology deal with the interrelations of the various processes which characterize living systems as well as with the relations of these processes to the environment of these systems. In this respect physiology and pathology are one. In experiment the physiologist usually brings about a pathic event to throw light on the processes of the healthy event. The only distinction as I see it (and this is not a thoroughly objective one) is that physiology deals with those processes which characterize the state of easy tolerance of living systems, while pathology concerns itself with those which lie outside this state. Ricker (151) insists that this distinction is based on teleological considerations and that physiology deals with the statistically normal processes and pathology with the abnormal. As pointed out before, the latter basis at present is not free from all subjective influences because the normal usually is determined by some one's judgment or ideal.

Further confusion in the use of the terms physiological and pathological arises from the fact that events which are pathic at a

certain level of organization often are considered physiological because they are usual or even because they are functional events in that they contribute to the development and persistence of the organism as a whole (107). This is illustrated by "physiological" loss of tissues, or cells, and by "physiological death." It is more consistent and objective, however, to consider these pathic events, recognizing that they are statistically normal and play a functional rôle in the development and persistence of the organism as a whole.

Another confusion of the terms physiological and pathological is found in the designation of certain plant diseases which are not incited by parasites nor by other obvious external agents, as "physiological diseases," with the intent of emphasizing that their symptoms are characteristically physiological, and that they are caused by disturbance in the physiological processes of the plant. This terminology has its seat in the distinction which arose between pathology and physiology during the early history of modern pathology, and which has been perpetuated in the organization of the federal and state research, teaching and service institutions and in botanical textbooks. Use of the term "physiological disease" in contradistinction to parasitic and non-parasitic diseases clearly indicates the overwhelming emphasis placed on the phenomenon of parasitism, in the definition of disease and of the field of pathology, and the neglect in teaching and research of the fundamental physiological aspects of even infectious diseases in the formulation of ideas and experiments. The effect of the germ theory on the ideas and development of plant pathology in the United States was even more pronounced than its effects upon human pathology, which Henderson (87) has described as follows:

> The science of pathology is notoriously backward and non-rational. During the epoch of pathological anatomy, from the taxonomical period of Morgagni and his successors to the cellular period of Virchow, this science no doubt held its own with the other medical sciences, but, upon the appearance of the bacteriological period in the history of pathology, an intellectual decline, as I think it may be fairly regarded, set in. In accordance with the simple views of Pasteur, the specific virus came to be regarded as the cause of each infectious disease and this view could hardly be questioned during the time of rapid progress in discovery of microörganisms. The hypothesis is roughly correct, but it has obscured the organic character of disease and the mutual dependence of the many variables which must be taken into account, if the state of the patient is to be understood. Yet the facts can never be quite overlooked by the experienced physician, for whom the complex reality is always present.

The deleterious effect of the germ theory on the development of plant pathology might have been avoided to a greater extent than it was in the United States, if the counsel expressed by Barnes (7) in 1899 had been heeded in the organization of botanical research and teaching and in the training of pathologists. In his vice-presidential address before the Botanical Society of America, Barnes, in discussing the relation of physiology and pathology, said:

> Closely connected with the study of the normal activities of plants are disturbances in the rate and character of function which are properly included under the term pathology. During the past decade very rapid advance has been made in a study of those pathological changes which are due to the presence of a foreign organism. Indeed, the phrase "diseases of plants" calls to mind almost exclusively the effects of parasites. . . . . Indeed, as compared with other fields, we are tempted to say that this has been overcultivated.
>
> There is thus one phase of pathology which has yet been comparatively neglected. . . . . We need also knowledge of those disturbed functions whose causes are dependent on other stimuli than the presence of a parasite. Some of these are doubtless internal and may long remain obscure. . . . . But unquestionably many plant diseases are due to untoward conditions

of the environment working sometimes through chemical, sometimes through mechanical, sometimes through ethereal stimuli. . . . . Thus the full study of pathology needs not only the examination of the parasitic diseases, but also a wide acquaintance with the proper activities of healthy plants in order to determine what derangements are produced in them by untoward circumstances and obscurer internal causes.

Such counsel has not been completely ignored, for nonparasitic diseases have been studied to some extent by pathologists, but even more by physiologists, horticulturists, and agronomists. Pathologists so far as permitted by interest, training and the organization in which they found themselves, have studied the fundamental physiological, biochemical, and biophysical aspects of infectious diseases. The situation now is apparently better in the United States than in Europe, for Morstatt(130), in his discussion of physiological pathology, based largely on conditions in Germany, says,

Obviously physiological pathology is at present the most important line of study to be followed in plant pathology and we see even now that its problems are being given detailed consideration in a few foreign countries. At present, however, we do not possess a comprehensive summary of the field, because pathology, in that it is not experimental pathology, is in fact one of the most neglected fields of botany, and the pathologist until now has been diverted by study of parasites.

Unlike plant pathology, human pathology possesses treatises by Ricker (151), E. Bauer (9) and J. Bauer (10, 11), devoted entirely to physiological pathology, and a treatise by Wells (184) devoted to chemical pathology.

Historically all striking deviations from the statistical normal, whether anomalies of form or of function, and irrespective of their relation to adjustment and persistence, have been considered part of the domain of pathology. Events incited by unusual factors have also been considered as part of its field. I believe something is to be gained in recognizing that plant pathology is a *system of knowledge dealing with events in the various levels of organization of plants which lie outside the range of easy tolerance of the organization; in other words, with the material basis, antecedents, consequences, and, in its practical expression, with the control of events during which the reactions of the organization exceed its limits of easy or ready adjustment to internal and external changes.* If the term disease is considered as synonymous with pathic event we may substitute it for pathic events in the definitions. Pathology consequently deals with the supremely critical biological question whether a living system will persist and reproduce and thus maintain the changing continuum of substance and process out of which it has arisen and which is momentarily expressed in it, in the form of the vortex or discrete dynamic entity or event which we recognize as the individual organism.

ETIOLOGICAL PATHOLOGY

Etiological pathology concerns itself with the antecedents or causes of pathic events. The term etiology or aetiology, means science of causes, and is derived from aitia (αἰτία), which means blame or cause. The notion of cause apparently originated in the field of legal procedure, where aitia or cause is a case, ground, or basis for an action. In so far as pathology approaches its subject matter with a dynamic point of view and aims to analyze each pathic event into progressively briefer part events and to consider these isolates in their temporal relations, it is etiological. In other words, every thoroughgoing spatio-temporal description of pathic events is etiological. In its etiological aspects, pathology, like physiology, is clearly a natural science, seeking experimentally to establish relations between antecedents and conse-

quents, and to classify these sequences. When it concerns itself with pathic events in all levels of organization of all living things it functions as a rigorously objective general or comparative pathology. In so far as pathology is interested in more than gross description of the static aspects of its subject matter, all of its analysis, description, or explanation is etiological. A pathology which aims to be a natural science is etiological throughout, and the term etiology really has no more significance in pathology than in the other sciences from whose vocabulary it was dropped long ago.

In the past, pathology, especially plant pathology, all too frequently has naïvely conceived the relations of antecedents and consequents as simple, and has not fully recognized that complexes of sequences are involved in practically every pathic event. Psychological and historical reasons for this will be discussed at the close of Part III. There has been a tendency to take the living organization which is the seat of the pathic event, as a "*given*" and to consider the external factors alone as "*the cause.*" In doing this the organization of the affected organism as a source of pathogenic factors has been neglected, even though pathologists, especially the germists, have given special consideration to susceptibility, resistance, and immunity.

In stressing the relation of processes, etiological pathology does not ignore form and structure, for it recognizes that, while the structures of an organism at any given moment are the resultants of changes and processes of previous moments, they also are the seat of the activities of that and subsequent moments.

Since geneticists, especially those who are physiologically minded, have concerned themselves with the genesis of many biological events which are recognized as pathic, and have succeeded in expressing the relation between these and genetic factors in quantitative terms, it is desirable to consider to what extent the concepts, propositions, data and methods of genetics may prove fruitful in etiological pathology.

III

The field of genetics is variously defined. Bateson, who coined the term, considered genetics to be the body of biological knowledge dealing with heredity, variation, development, and evolution. Accordingly genetics seeks to explain the resemblances and differences in organisms related by descent. Some consider the field more narrowly to be the study of the distribution of genes in the transmission of genes from parent or parents to offspring. There is, however, growing appreciation that if genetics is to avoid sterility it must include developmental physiology and morphology and concern itself not only with how the characteristics of parent and offspring are related, but also how the factors that play a rôle in conditioning the characteristics of the mature individual lie latent in the initial cell, and how they function as the organization of the individual develops. Separation of these methods of analysis has been more sharp in the zoölogical than in the botanical field.

Genetical studies have clearly established that the hereditary or genetic constituents of a living organization play a rôle in all events, both healthy and pathic, throughout its life cycle. Even duration of the cycle of individual life is genetically conditioned, as was first demonstrated by Hyde (94) and Pearl (140).

MENDELIAN, CHROMOSOMAL, OR GENIC INHERITANCE

Experiment has led the geneticist to assume that the material basis of inheritance consists of material unit particles, the genes, which are characterized by power to divide

or duplicate themselves and the capacity of each to retain its specificity through many generations.

Two kinds of inheritance are recognized: Mendelian, and non-Mendelian. The former is the prevailing (certainly the more obvious) type in those organisms whose cells are characterized by a definite nuclear organization and by mitotic and miotic divisions. Cytological and genetic investigations have established that the chromosomes are the carriers of the genes in Mendelian inheritance, which consequently is also known as chromosomal inheritance. On the basis of much experimental work geneticists also assume that each chromosomal gene is localized in a definite point in the chromosome. The term gene is quite generally applied only to the hereditary units so carried in the chromosomes, and hence Mendelian inheritance is often designated as genic inheritance. The chromosomal genic constitution of the haplophase and of the diplophase is designated as the "genom."

The theory of the Mendelian gene has been formulated as follows: (1) The hereditary events or characteristics of individuals are conditioned by genes, which in the zygote and diplophase are paired as allelomorphs, and which occur in a definite number of linkage groups in the chromosomes derived from the parents or parent. When the allelomorphs are identical the organism is said to be homozygous for the characteristics or events conditioned by that gene, and if they are unlike, it is said to be heterozygous. If one allelomorph is as effective as the pair, it is said to be dominant, and its partner recessive. Sometimes more than two genes can be allelomorphic (multiple allelomorphism). (2) The allelomorphs segregate when the germ cells mature so that each germ cell comes to possess only one allelomorph of each pair contained in the zygote or the diplophase as formulated in Mendel's laws. This applies also to the haplophase if one occurs. (3) The genes of different linkage groups assort independently in reduction division as expressed by Mendel's laws. (4) An orderly interchange, or crossing-over, of genes takes place usually between the constituents of homologous linkage groups. (5) The frequency of crossing-over furnishes evidence of the linear arrangement of the gene in each linkage group and of the relative position of the genes with respect to each other (53, 64, 80, 126, 129).

NON-MENDELIAN INHERITANCE

a. *Viruses and Schizophytes.*—Minute entities which have the characteristics of living organizations, such as bacteriophages and filterable viruses, may be considered single genes; and organizations without definite nuclei, such as bacteria and the blue-green algae, may be considered to bear a number of one kind or relatively few kinds of genes, irregularly distributed in the cytoplasm. Since the hereditary units of these organisms are not chromosomal and inheritance is non-Mendelian, these hypothetical units are not generally recognized as Mendelian genes (199).

b. *Cytoplasmic Inheritance.*—The term non-Mendelian inheritance is usually used in the sense of cytoplasmic inheritance. Practically all reported cases occur in a few plants, so that cytoplasmic inheritance is generally assumed to be relatively rare and of little import for a general theory of genetics. A few geneticists at the present time, however, in attempts to correlate the data of genetics and developmental physiology have come to the conclusion that cytoplasmic inheritance, while less obvious than Mendelian heredity, is fully as general and as significant. While most of those who admit that

cytoplasmic inheritance occurs postulate that its material basis consists of cytoplasmic genes, Correns (52) and Wettstein (188, 190, 191, 192) conclude that an invisible cytoplasmic structure, a genetically differentiated plasma, the plasmon of Wettstein or idio-cytoplasm of Correns, exists in every cell. This is assumed to halve in each cell division and then to grow to full size again (52). This genetic cytoplasmic system, like the chromosomal genes, is assumed to play a rôle in conditioning each and every event and characteristic of an organism.

The assumption that cytoplasmic inheritance occurs is based on the observation that the characteristics of hybrids in certain reciprocal crosses are conditioned by the maternal cytoplasm. It is also based on Harder's defect experiments with Basidiomycetes (85), in which he prevented the second nucleus of a binucleate hybrid cell from entering the cell, which, however, contained the plasmas of both parents. In this manner he obtained characteristics which are conditioned by the plasmas independently of the nuclei. The literature dealing with these new tendencies of genetic thought is critically discussed for plant material by Correns (42, 52), Demerec (59), Lehman (110), Oehlkers (136), and Wettstein (191).

THE GENETIC SYSTEM; CORRELATIVE INFLUENCE OF GENOM AND CYTOPLASMA

By the genetic system or constitution of an organism is meant not only the hereditary units or genes which it receives from its parent or parents, but also the individually characteristic manner in which they are associated or related to one another to produce the particular organization encountered at any given instant. Most current genetic theories assume that the nucleus monopolizes the genetic system. However, in the light of recent experiments, Baur (13), Harder (85), Oehlkers (136), and Goldschmidt (81) recognize the existence of cytoplasmic genes. Correns (42, 52) and Wettstein (188, 190, 191, 192) think it necessary to assume not merely that single cytoplasmic genes are part of the genetic system of an organism, but that this includes, in addition to its genom or genomes, a cytoplasmic genetic system (plasmon or plasmones) which is the carrier of developmental processes, whose intensity, direction and speed are guided by the chromosomal genes on the one hand and by external factors on the other. Lack of harmony between genomes and plasmones is assumed to lead to pathic development or to death. Changes by mutation in the one, therefore, necessitate appropriate changes in the other if the organism is to be healthy.

CORRELATIVE INFLUENCES OF GENETIC SYSTEM AND CYTOPLASM: RÔLE OF PAST HISTORY OF CYTOPLASMIC SYSTEM

That correlative influences exist between the genetic system and the non-genetic part of the cytoplasm is assumed by many who do not find it necessary to assume the existence of a cytoplasmic genetic system. The cytoplasm is recognized as the obligate medium or environment in which the genes come to expression. It is also recognized that most plasmas are unfavorable (pathogenic) for foreign genetic systems. It is contended, however, that while the cytoplasm at any moment is a part of an unbroken continuity of cytoplasm, this continuity is of a different order than the genetic continuity of the relatively unchanging genes. The continuous thread of cytoplasm is synthesized and changes from moment to moment during development under the past and present influence of the genes, and plays its major rôle in conditioning whether and how the genes shall act at a given instant (Fig. 3).

As it changes, in the course of development, under the influence of the genes and of the environment external to the cell, the cytoplasm is assumed to provide a different medium at successive instants of time in which the same genes condition different events so that a relatively small number of genes in mutual interaction with a small amount of different cytoplasms play a rôle in conditioning all events or characteristics of an organism. According to this hypothesis all of the genes coöperate to condition the development of the organization of a plant throughout its entire life cycle, and a single gene plays a rôle in many events. Furthermore, any given event or characteristic may be conditioned by the coöperation of several genes or by the action of one gene. It is recognized, therefore, not only that correlative influences exist but also that past events in the cytoplasm are a factor in development. The latter factor is designated as "past history" (Figs. 2, 3).

FACTORS IN CELLULAR DEVELOPMENT

If the development or other events of the cell are analyzed into part-events it is apparent that a given part-event at a given time and place is conditioned by four factors. One of these is the influence of the local genetic system, which comprises (a) the local chromosomal genes and (b) either the local cytoplasmic genes, or a cytoplasmic genetic system (the plasmon of Wettstein). A second factor consists of correlative influences of the constituents of the genetic system on the one hand and of the non-genetic part of the cytoplasm on the other hand. A third factor is the influence of the past history of the non-genetic constituents of the cytoplasmic system, and a fourth the immediate influence of the external environment (Figs. 2, 3).

What a gene or a plasma conditions can be established only by events or characteristics that become apparent through gene or plasma differences, i.e., when one gene or when the plasma is changed, differently linked, combined, or absent. Consequently pathic events, which usually are striking and obvious, have played an extremely important rôle in genetic analysis. Genetic analysis, on the other hand, has revealed that the chromosomal genetic system, the cytoplasmic genes, and the non-genetic cytoplasm, play rôles as direct or indirect pathogenic factors. In the following discussion the pathogenic rôle of the chromosomal genes and allied phenomena will be taken up first. Following this the pathogenic rôle of the cytoplasmic constituents will be discussed. This will lead to a brief discussion of the influence of past history of the cytoplasm and of correlative influences between cells and other parts of a multicellular organism as pathogenic factors. The rôle of the external environment, including foreign pathogenic organisms, will be touched only in passing.

No attempt will be made to discuss the literature bearing upon the rôle of the past history and correlative influence factors. Such systematized literature as exists concerning the former factor, and the great body of that bearing on the latter are summarized by Pfeiffer (141), Küster (107), Goebel (79) and Child (31, 32).

PATHIC CHROMOSOMAL GENES; LETHALS, SEMI-LETHALS AND PATHIC EVENTS

Since genes at present are known only by their effects nothing can be directly known about their pathology. A gene which has its molecular structure or organization disrupted or so altered that it is not able to adapt itself readily to its environment as measured by its stability, is a pathic gene. In this sense many mutations must be pathic events at the gene level of organi-

zation. Correns (50, 52) not only uses instability of cytoplasm as a criterion of pathic cytoplasms but also considers instability or high mutability of genes as a criterion of the pathic at the gene level. He considers the highly mutable gene of *Capsella bursa pastoris albovariabilis* as a pathic gene (47). Another case of a highly mutable gene in plants is that described by Emerson (67, 68, 70) for the pericarp color of *Zea mays*. Demerec (59) also assumes a highly mutable gene to explain certain chlorophyll deficiencies in plants.

While we are not in a position at present to say much about either the healthy or the pathic constitution and behavior of genes, we are in possession of a great body of data which bears on their rôle as pathogenic factors. In the course of genic analysis it has been established by a vast body of data that many genes occur which alone, or as a pair of allelomorphs, or in combination with still other genes, or with suitable cytoplasm or environmental factors, directly or indirectly incite or condition pathic events of all possible grades of severity. Most striking of these are the so-called lethal, sub-lethal or semi-lethal genes, which, beginning with the earliest stages of the embryo, condition death at various stages of development of the individual. In some instances embryoless seeds are found. Renner (145) demonstrated that embryoless seeds and seeds with various embryo and endosperm defects are genically conditioned in crosses of *Oenothera lamarckiana* and *O. biennis*. (In the following discussion the terms genic and genically are restricted to chromosomal genes.) Similar phenomena have been demonstrated for other plants. Beginning with a report by Jones (98,99) of strains of *Zea mays* with genically conditioned defective grains, American geneticists have brought to expression an imposing array of pathic embryos and pathogenic endosperms which are conditioned by recessive lethal and sublethal genes in the diplophase. The most striking is the case of complete failure of embryo development (Demerec, 56), in which apparently four pathogenic genes are present for the condition "germless." Other defects are "shrunken endosperm" (Hutchinson, 93); "mealy endosperm" (Mangelsdorf, 122); "scarred endosperm" (Eyster, 72); "sweet defective—flint defective" (Lindstrom, 115); "miniature germ" (Wentz, 185); "concave defective" (Wentz, 187); and "barren sterile" (Woodworth, 197). In fourteen strains of *Zea mays* with defective grains Mangelsdorf (123) found that fruits which weigh from 1 to 20 per cent of the weight of healthy grains either are devoid of embryos or contain embryos and endosperms so defective that no germination occurs; fruits weighing 30 per cent or more of the weight of healthy ones germinate and produce seedlings with decreasing frequency of death as the weight of the fruits approaches that of the healthy seeds. There is thus a series graded from complete lethality to complete developmental capacity in the diplophase which is genically conditioned.

Lethal and sub-lethal effects in the diplophase are especially marked in pathic events characterized by chlorophyll deficiencies. In *Antirrhinum*, Baur found a condition, *albida*, characterized by complete failure of chlorophyll development and death of the seedlings in a few days after germination, as well as an *aurea* type with golden leaves, in which death of the embryo or of the seedlings occurs when they are two to three days old (12). In *Mirabilis jalapa*, Correns (41) found a *xantha* type, which has 5 per cent of the chlorophyll content of the healthy strain (*typica*) and is non-viable. It may survive and set seed if grafted on healthy plants.

Beginning with a report of abnormalities by Emerson (65, 66) in 1912, a long list of chlorophyll deficiencies has been reported for *Zea mays* in the seedling and mature stages, notably by Miles (128); Lindstrom (112, 113, 114, 116); Bach (6); Kempton (103); Anderson (2); Demerec (54); Collins and Kempton (37); Eyster (74); Trajkovich (174); Jenkins (95); and Stroman (168). In four types of these, albino, pale green 1, pale green 3, and pale green 4, the seedlings die early. In some they die before maturity, while in others these pathic plants, though very weak, can be made to reach maturity and fruiting if special precautions are taken. Eyster (75) has reported that inability of the embryo of *Zea mays* to utilize carbohydrates of the endosperm is genically conditioned by a recessive gene, and that failure of the embryo to go into a rest period in certain strains is conditioned by two independent recessive genes (73).

Less severe pathic characteristics occur which are genically conditioned. In *Antirrhinum*, Baur (12) found a type, *crispa*, with irregularly wavy leaves, in which there is partial failure of the epidermis to develop. The plants are weaker and small but may mature and flower. When the dominant gene which conditions this is homozygous it has a lethal effect. In *Mirabilis jalapa*, Correns (43) found weaker plants ("*sordida*") with a dirty green color and with light brown spots on the surface of the leaves ("sordago disease"). In these spots the palisade tissue is first altered by swelling of the cells, which press on the epidermal cells so that both die, forming necrotic areas. This is conditioned by a single recessive gene and was described by Correns as an inherited disease. Finally in *Zea mays*, the conditions, "blotching," characterized by yellow blotches and later by dead spots (Emerson, 69), and "dead leaf" margins (Kempton, 104), in which some of the tissues die, are determined by single recessive genes.

STERILITY AND SEMI-STERILITY AS PATHIC EVENTS

The rôle of the genic constitution of plants as a pathogenic factor is clearly illustrated by the so-called sterility, substerility and self-sterility phenomena in both the haplo- and diplophases (24, 155). Pollen sterility in the form of non-functioning pollen grains is genically conditioned in *Oenothera* according to Renner (145, 146, 147), and in *Nicotiana* according to Goodspeed and Clausen (82), and East (61). Sterility phenomena in *Melandrium*, surmised by Correns (39) to be characterized by feeble growth of the pollen-tube, were later established to be genically conditioned (46). Belling (19) has demonstrated for *Stizolobium* crosses that sterility in embryosacs occurs which is genically conditioned after selection of megaspores for development has taken place; and Woodworth (197) has reported that "barren sterile" in *Zea mays* is genically conditioned. Failure to produce pollen, "male sterile," has been reported by Eyster to be genically conditioned in *Zea mays* (71).

Reproductive sterility is characterized by failure of the plant to produce flowers, stamens, carpels and micro- or megaspore mother cells. Baur (17) found a genically conditioned sterility in the *sterilis* race, a mutant of *Antirrhinum majus*, in which flowers with only leaflike structures are produced. He also found a genically conditioned failure of healthy development of the anthers and pistils in *A. globosa*, *globifera*, *nicotianoides*, and *ericoides*. Similar phenomena were found in *Petunia* and *Matthiola* by Saunders (160, 161, 162) and by v. Ubisch (175) for *Petunia*. Destruction of the sporogenous tissue in the anther, destruction of the pollen, and failure

of the pollen to develop were reported to be genically conditioned in *Zea mays* (Eyster, 71), in *Epilobium* (Schwemmle, 164), in *Bryonia* (Tischler, 169), and in *Oenothera* (Oehlkers, 134, 136). Genically conditioned failure of the pistil to develop was reported for calycanthema races of *Campanula medium*, and for *Mimulus tigrinus* (Correns, 40).

The phenomena of sterility grade imperceptibly into those of self-sterility. One speaks of self-sterility when plants which produce micro- and megahaplonts, functioning in cross pollination, are not able to set seed when pollinated with their own pollen or with that of plants of the same clone. The pollen-tubes either are not developed or fail to reach the embryo sac. Apparently inhibitory factors are involved which are multiple allelomorphs but which must be considered heterozygous. When a pollen tube bears the same inhibitory factors as the style or stigma, sterility occurs; if the factors are unlike, fertility is the result. Here one deals with factors potentially pathogenic for the haplophase in certain biologic environments. These phenomena have been demonstrated to have a genic basis in *Nicotiana* by East (60, 61, 62, 63) and in *Veronica syriaca* by Lehman (108) and Filzer (77). Similar phenomena apparently also occur in Hymenomycetes in the phenomena which have received the absurd designation of multipolar- or multi-sexuality (Oehlkers, 136), (Link, 118).

INDIRECT PATHOGENIC RÔLE OF GENIC CONSTITUTION

In addition to this direct rôle as a pathogenic factor, the genic constitution also plays a rôle as an indirect or contributory factor in that it conditions events or characteristics which make it possible for other pathogenic factors to incite pathic events directly. Such a rôle is seen in the genically conditioned susceptibilities to internal and external factors, to which plants without these susceptibilities do not react pathically. Correns (44) described a genically conditioned susceptibility to low temperature for *Mirabilis jalapa forma delicata* and Honing described a genically conditioned susceptibility to the light and temperature of Sumatra in Dehli *Nicotiana* (88) which does not come to expression when the plant is grown in Holland. Oehlkers (133, 134) reports a genically conditioned susceptibility in *Oenothera* hybrids. Åkerman (1) reports that *Avena sativa* hybrids are genically conditioned to die in bright light.

The well established cases of genically conditioned susceptibility to infection and infectious diseases incited by viruses (bean mosaic), bacteria, fungi, round worms, and insects are too numerous to cite. A paper by Walker (181) contains a summary of striking instances of genically conditioned susceptibility and resistance in infectious diseases of plants. Recently, Fischer and Gäumann (78) have systematized the vast body of data bearing on this subject. The bibliographical monograph by Matsuura (124) includes many instances of genically conditioned susceptibility to pathic events in plants reported up to 1925.

In a coöperative venture it was demonstrated that the susceptibility to the fungus *Colletotrichum circinans* described for white onions by Walker (182) is due to the lack of protocatechuic acid, which was isolated from pigmented, resistent onions, chemically identified, and demonstrated to be absent in white onions by K. P. Link and Angell (121). Rieman (152) demonstrated the presence, absence, and degree of pigmentation to be genically conditioned. Furthermore, the formation in red and yellow onions of the pigments of which protocatechuic acid is either a building stone or a degradation product,

while genically conditioned, is also conditioned by light.

INHERITANCE AND ORIGIN OF PATHIC AND PATHOGENIC GENIC CONSTITUTIONS

Observation and experiment have shown that the laws which express the inheritance of pathic and pathogenic genes are in no way different from those for healthy and non-pathogenic genes. They are the rules which apply to chromosome and gene distribution in the passage of chromosomes from parent or parents to the new individual. Some pathic events are conditioned by a single gene if this is dominant; most of them by allelomorphs, since most pathogenic genes are recessive; and others by coöperation of several genes. The gene for the *crispa* state in *Antirrhinum* is dominant; in the homozygous state it is lethal. *Sordida* differs from the healthy *Mirabilis* plant by a single gene; apparently four genes are involved in "germless" of *Zea mays*; shrunken endosperm, scar, and miniature germ in corn are conditioned by one recessive gene each; at least ten genes are involved in "albino" corn seedlings; five of them are independently inherited; four of them form two sets of duplicate genes, and at least one additional gene can be obtained from a set of triplicate genes. Linkages of pathogenic genes have been reported for the sweet defective and sugary endosperm in *Zea mays* (Wentz, 186).

Experiment has shown that pathic and pathogenic genic constitutions arise abruptly by gene (factor) mutations from healthy and non-pathogenic genes, and that the overwhelming number of detected gene mutations involve pathogenic genes. This is not surprising, for the probabilities are that random change in a complicated mechanism will be for worse. Most gene mutations presumably produce such small effects in the organism that these seem negligible and may be useful to the species only after untold numbers of recombinations. If not of this type they usually are definitely pathogenic. They seem to arise by inactivation of the mutated genes, for they usually cannot express themselves when paired with their non-mutated allelomorph, i.e., they are recessive. Some mutations possibly also involve activation, or increased activation so that the changed gene expresses itself when paired with the non-mutated gene. Such genes are dominant. There are relatively few dominant pathogenic genes. If it should develop that gene mutations may be induced in nature as in the laboratory, by irradiation, then it would seem that these mutations are events in the genes which are not pathic but which are characterized by such a change in the organization of the gene, possibly by inactivation, that it either cannot carry the development of a characteristic as far as a healthy gene, as in recessive genes, or that it carries development further than it did before it mutated, as in dominant genes. Considered from the genic level such genes are abnormal statistically, but normal physiologically, in that they are adapted and persist. They may be said to be defective, malformed, or imperfect, having lost some parts. They are not pathic. To be pathic they must be so poorly adapted that they do not persist. Considered from the level of organization of the individual and of the species, these mutated genes are pathogenic for the former when they are lethal or sublethal, and for the latter when they are sterility genes.

Pathogenic mutations also occur by changes in the organization of the genic system, genom mutations such as changes in number of sets of chromosomes (polyploidy), addition or loss of one or more single chromosomes (heteroploidy), changes in the constitution of chromosome

sets (unbalanced sets) and changes in the structure or composition of particular chromosomes (crossing over). In some cases the new constitutions are pathic and do not persist. In *Oenothera*, chromosome aberrations arose from fertilizing *O. semigigas*, which has three sets of chromosomes (triploid), with pollen of *O. decipiens*, which has one set of chromosomes (haploid), and resulted in high lethality in the embryos with 17–21 chromosomes. In other cases the new constitutions are not pathic for the individual but pathogenic from the species point of view, i.e., condition sterility by virtue of chromosome aberrations. If the chromosome constitution is such that extra chromosomes occur which are distributed by chance in reduction division, the pollen with the extra chromosome dies, resulting in partial pollen sterility. Such events occur in *Oenothera* and *Datura* (Van Overeem, 139; Boedjin, 27; Belling and Blakeslee, 20).

PATHOGENIC RÔLE OF NON-NUCLEAR GENETIC FACTORS (PLASMA AND PLASTIDS)

Hereditary chlorophyll deficiencies which apparently are not chromosomally conditioned occur quite frequently in plants. They range from mere mottling to pure white plants which are so pathic that they die unless the affected plant is grafted into a green one. In cases with less complete whiteness the affected plant, branch, leaf or leaf area is not as vigorous as the green one and dies earlier. In other cases the affected islands are so small that by virtue of association with healthy green tissues they persist as long as do the healthy tissues. Correns (41, 42) obtained a mutant with green, variegated and pure white branches in hybrids of *Mirabilis jalapa* which he designated as *albomaculata*. By selfing or crossing flowers of variegated branches or plants he obtained variegated, green, pale green and white seedlings. Whatever the cross, the seedlings always were of the same color as the maternal plant. If a flower of a green branch or plant was hybridized with pollen from a white branch or plant, green seedlings resulted, whereas the reciprocal cross ♀ white × ♂ green gave whites only. Similar cases of maternal inheritance are reported by Correns for *Stellaria media*, *Senecio*, *Taraxacum*, *Hieracium*, *Arabis* and *Mesembryanthemum* (48, 49); by Baur for *Antirrhinum* (13), Shull for *Melandrium* (163), Gregory for *Primula* (83), Anderson (2) and Demerec for *Zea mays* (59), and Chittenden (33) for *Hydrangea*. Maternal inheritance also occurs in *Humulus japonicus albomaculatus* as reported by Winge (195), but the flowers of variegated plants produce only variegated seedlings.

Baur (12) investigated the periclinal chimera *Pelargonium zonale albomarginata*, which produces not only albomarginate but occasionally also pure white and pure green branches. Reciprocal crosses in both directions between flowers of white and green branches give variegated plants. Inheritance consequently is not maternal only.

Hypotheses advanced to explain the genetics and developmental physiology of these situations are of significance to the pathologist. Correns (41, 42, 52) postulates for *Mirabilis* that a pathic state (*Kranker Zustand*) of the cytoplasm is involved which prevents healthy plastid development and persistence. In other words the plasma plays a pathogenic rôle with reference to the plastids and the plastids with reference to the cell as a whole and to tissues, organs, and entire plants. He also chooses to interpret the transmission of this pathic cytoplasm not as inheritance but as direct transmission of a state. He assumes further that the plasma of the zygote which produces varie-

gated plants is in a labile state and as a consequence of cellular correlative influences or of external influences may be fixed in a healthy or a pathic state at various stages of cell and tissue development, thus leading to patches of green and white.

Baur (13), while he grants that Correns' interpretation may apply to the mottled types of variegation, assumes that in other cases abnormal and normal plastids occur which are carried by the micro- and megagamete or by only the latter, as the case may be, and are then distributed inequally during cell division so that eventually pure white and pure green tissues result. Correns gives limited assent to Baur's interpretation. He prefers to designate the genetically non-green plastids as pathic and the green as healthy. Certainly the non-green always are potentially pathogenic and actually so in many cases for cells, and higher levels of organization when they occur in sufficient number. Winge (195) assumes that the potentially pathogenic abnormality lies in the plastids in those forms which produce green, white and mottled individuals, whereas in the forms which produce only variegated shoots it resides in the plasma.

Eyster (76) seeks a way out of the troublesome problem of variegations by assuming that while the gene is the unit of inheritance in meiosis it is not the unit of segregation in mitosis. He postulates that the gene is an organization of self-propagating gene particles, genomeres, which assort at random at mitosis. He also assumes that genes which are composed of more than one kind of genomere are mutable, and that in variegated strains, pigment is produced when a gene has a certain threshold value of pigment producing particles. If his explanation is to apply to these variegations which are maternally inherited he must assume that cytoplasmic genes condition the discoloration and that genomere assortment is a potential pathogenic factor.

Demerec (59), in reporting a case of maternal inheritance of chlorophyll deficiency in *Zea mays*, reviews the literature in which attempts are made to explain maternal inheritance on a chromosomal basis, and suggests an additional hypothesis. He assumes that a dominant mutable gene might be involved which has a highly increased frequency of mutation when it comes in contact with other than parental cytoplasm. In suggesting a chromosomal explanation of *Pelargonium zonale*, Demerec again assumes that a mutable gene might be involved whose mutability for white is increased when brought by the pollen nucleus into other than maternal cytoplasm. He considers the genetic evidence insufficient for any of the explanations offered for maternal variegations.

More recently, however, Rhoades (150), in reporting cytoplasmic inheritance of male sterility in *Zea mays*, considers that cytoplasmic inheritance has been clearly demonstrated for certain chlorophyll characters. He concludes that the sterility studied experimentally by him is determined entirely by non-nuclear elements of the maternal gamete. He carried his cultures past the F_2 generation, meeting the criticism of Demerec (59), and demonstrated that nine of the ten linka gegroups were free from any factor or factors that condition the sterility.

PATHOGENIC RÔLE OF INTRACELLULAR CORRELATIVE FACTORS

In assuming that the pathogenic factor is genetically fixed in either the cytoplasm or plastids, Baur's explanation postulates independence of nucleus and plasma. Correns' explanation, in so far as it assumes a labile state of cytoplasm, involves the assumption that nucleus and cytoplasm are closely correlated, and that in the dis-

eased state the control which the nucleus usually exerts over plastids is lost. Noack (131) joins Correns in this view and elaborates the idea that a disharmony of the processes conditioned by both nucleus and cytoplasm is the pathogenic factor in variegations. Oehlkers also (136) favors this interpretation. Demerec (59) assumes that correlative influences exist between chromosomal genes and cytoplasm.

Further evidence that correlative influences between nucleus and cytoplasm may play a pathogenic rôle is found by some investigators in reciprocal crosses of certain plants. The degree of fertility in such plants apparently is conditioned by the degree of coöperation of nuclear genes and cytoplasm. Contributions bearing on this phenomenon have been made by Jones (102) for *Digitalis*, by Lehman (110), Renner (145, 146, 147, 148, 149), by Lehman and Schwemmle (109), and Schwemmle (164) for *Oenothera* and *Epilobium*; by Wettstein (188, 189) for hybrids of species and genera of mosses; and by Chittenden and Pellew (34) for *Linum*.

While none of the hypotheses advanced to explain the matrocliny which manifests itself in pathic variegations and in sterility is thoroughly satisfactory genetically, these hypotheses are of significance in etiological pathology. They imply that extra-chromosomal genetic constituents and disturbed correlative relations between the chromosomal genes of a cell and its cytoplasmic constituents play pathogenic rôles in pathic events in cells and higher levels of organization. They also imply that mutations which occur in the genetic constituents of the cytoplasm are potentially pathogenic, and that if Eyster's interpretation is valid, somatic segregation is a potential pathogenic factor.

PAST HISTORY AS A PATHOGENIC FACTOR

The discussion of the rôle of the cytoplasm during and after development indicates that the cytoplasm is a potential pathogenic factor by virtue of its influence on the genetic system. It was also intimated that the past history of the cytoplasm plays a rôle in conditioning an event at a given time within a given cell.

The cytoplasm at any moment is the expression and repository of past events in living organisms and thus is the substance which plays a rôle as the factor of past history. The cytoplasm is not only the seat of change but it also changes during development and has definite determination points at which definite events occur in its development. The cytoplasm and its products are essentially the material basis of the vast display of form and structure that characterizes organisms, that is, of their phenotypic aspect.

What a cell can do at any given moment, after the earliest stages of development, is largely conditioned by what has gone on within and what has been done to it previous to that moment. With the passage of time and of events in the course of development and differentiation its capacity to react and adjust itself changes and diminishes. Finally the cytoplasms become so changed, so differentiated, so unable to respond adaptively that senescence and death become increasingly inevitable. The past history factor thus plays a rôle as a direct pathogenic factor in cells (Child, 30). The "ontogenetic metabolic drift" reported by Blackman, Carré, Haynes, Kidd and West (26) is a case in point (Figs. 2, 3).

The indirect conditioning or contributory effect of the past history of a cell or tissue as a pathogenic factor, is seen in the increased susceptibility of aging cells to pathic events incited by stimuli which do not elicit such events in the cell or tissue while young. The internal breakdown of apple and pear tissues and scald in apples reported by Brooks, Cooley and Fischer (28) are cases in point. There are

innumerable illustrations among non-parasitic diseases of plants which show that past events condition susceptibility or resistance to subsequent events. The rôle of past history in the occurrence or non-occurrence of pathic events is illustrated by the phenomenon that plants which are not hardened by exposure to moderately low temperatures often succumb to subsequent low temperatures which are not lethal for the hardened ones (Harvey, 86; Maximov, 125; Rosa, 154) (Figs. 2, 3).

Plants do not show such striking illustrations of the influence of past events in the form of infections as do animals with their great display of spontaneously and artificially acquired immunity to infectious diseases. They do, however, illustrate the influence of past events, such as fertilizing, watering, and exposure to temperature, upon susceptibility and resistance to infections. The literature bearing upon this field has recently been summarized by Vavilov (177), Walker (181), and by Fischer and Gäumann (78).

All phenomena of physical and chemical pathic allergy are excellent illustrations of the rôle of past history in pathic events. The literature bearing upon this field for animals has recently been summarized by Coca (35).

EXTRA-CELLULAR CORRELATIVE INFLUENCES AS PATHOGENIC FACTORS

The analysis of pathogenic factors so far has concerned itself largely with events within cells. If the pathic event to be analyzed occurs within a multicellular organism another potential pathogenic factor must be considered, namely that of the influence of other cells or parts of the organism upon the cell or part in question. This is the factor of correlative influence, or of internal environment. Whether factors within an organism but external to a given cell, tissue or organ are to be considered external or internal environmental factors depends upon which level of organization is used as point of reference. In our analysis we shall designate as external factors only those which are outside the organism.

It has been previously pointed out that correlative influences exist in a single cell between its various constituents, between genes, gene groups, chromosomes, nucleus and cytoplasm and membranes. A disharmony in the genetic system, such as an extra chromosome, may constitute either a pathic event for the cell or a pathogenic factor. In fact, the functioning as a single system of the assemblage of genes and cytoplasm which enter into the genetic system of a cell is largely a matter of correlative effect, and in a certain sense the harmonious functioning of correlative influences is the criterion of a healthy cell organization, while a disharmony is the criterion of a pathic event.

In addition, correlative influences play a tremendously important rôle in development and persistence of a multicellular organism. Here, as in the cell, the action and effects of correlative influences between cell and cell, tissue and tissue, and organ and organ are the very keystone of what is called the organization of living things. While all cells of a given plant usually receive the same complement of genes, they do not all differentiate and mature alike. Which of the many potentialities locked up in a genetic constitution will come to expression, is determined for a cell or group of cells largely by correlative influences. These may be their relative positions, mechanical stimulation arising out of this position, chemical stimulation by products of the immediate and distant neighbors, or stimulation arising out of electrical and radiation relations with its neighbors. By virtue of them tissues and organs manifest self-differentiation. These correlative influences apparently condition that a cell gives up its toti-

potency with the result that different tissues arise out of the same meristem. When in a wound, some neighboring cells are removed from a differentiated parenchyma cell, this cell may give rise to one which in turn gives rise to a callus tissue. This callus tissue may differentiate into all possible sorts of cells, such as callus tracheids, callus parenchyma, cambium, all xylem and phloem elements, periderm, and fibers, and in time may form buds, shoots, and roots.

Because of the low order of integration in the plant body with its comparatively independent organs which are but slightly differentiated functionally, the rôle of correlative influences it not as apparent nor as easily demonstrated as in the mammalian body with its special glands, endocrine secretions, and circulatory and nervous system.

Correlative influences undoubtedly play a rôle in the normal pathic events which occur in the self-differentiation of a living totipotent cell into a dead, protoplasm-free, tracheid, trachea, fiber, or cork cell. In other words, correlative influences, or the internal environment, may play a direct pathogenic rôle. This is further exemplified in the casting and shedding of tissues, such as the endodermis, cortex and epidermis in root and stem development, and of reproductive structures, such as young fruit in the absence of fertilization. An illustration of the direct pathogenic rôle of correlative effects is found in the death of epidermal cells or of the guard cells in the periclinal chimera, *Laburnum adami*. The subepidermal cells derived from *Laburnum vulgare* seem to exert a pathogenic influence on the epidermal cells derived from *Cytisus purpurens* (29). This and other chimeras and grafts show, however, that the genetic constitution or local gene effects play a rôle that predominates over correlative influences. According to Hutchins (92) in the phony disease of the peach the root of the tree exerts a pathogenic influence upon the development of the top of the tree. This apparently is a case illustrating the pathogenic rôle of correlative influences.

In addition to their direct rôle, correlative influences also play an indirect pathogenic rôle. This is seen in the differential reaction to injurious gases by tissues near the veins of leaves. Guard cells are more easily obliterated than the other epidermal cells, while these are more susceptible to the pathic effects of low temperatures, acids, alkali, alcohol, ether, illuminating gas and ultraviolet light. The pathogenic rôle of correlative influences may underlie other cases of cellular and tissue susceptibility (Figs. 2, 3).

THE EXTERNAL ENVIRONMENT AS A PATHOGENIC FACTOR

Experiments, especially with material of the same genetic constitution, that is, with pure lines and clones, in different environments, have clearly proved that the external environment is a developmental factor in the organization of living things, and that its various constituents, non-living and living, play either a direct or an indirect rôle in pathic events. The influence of the external environment may be so severe that it directly leads to pathic events in the developing or mature organization, or if less severe, may condition development of an organization which is abnormally susceptible to pathic events.

It may also play such a preponderant rôle, as in violent injuries, that the organism is catastrophically overwhelmed and its organization plays such a subsidiary rôle that internal factors may be virtually ignored. This phase of the etiological complex is so obvious and well understood that no attempt will be made to discuss it here. In so far as it concerns itself with

non-living external factors it has largely been investigated by plant physiologists and ecologists, and to some extent by plant pathologists, but no summary of the extremely diffuse literature is available at present (Figs. 1, 2, 3).

If one of the significant external factors in a pathic event is another organism, as in an infectious disease, this organism, which itself is an event, can for any given instant be analyzed into its conditioning factors just as we have analyzed the pathic event. It is a well established fact that the behavior of an infective agent in relation to the infected organism is conditioned by immediate external factors, of which the organization of the host by virtue of its susceptibility or resistance is a very important one. It is also well known that external factors, such as light, moisture, and temperature, are important conditioning factors, directly, in determining the presence and abundance of the parasite, and indirectly in conditioning its infectivity. Furthermore, the organization of the parasite is a conditioning factor. This, as in the host, is the resultant of the interplay of its genetic constitution, of correlative influences, of past history and of the environment. It is well known that the infectivity of an organism (as measured by its relative ability to infect) and its virulence (as measured by its capacity to incite pathic events) can be increased by repeated passage and other methods. The literature bearing on this phase of etiological pathology has been summarized and systematized by Fischer and Gäumann (78) (Fig. 3).

THE ETIOLOGICAL COMPLEX

We may summarize the data presented by stating that every pathic event arises out of the complex of conditioning and inciting or releasing factors. The constitution of the affected organism is part of this complex; often a constant one, but nevertheless, an essential part. By constitution I designate not only the parts, i.e., structures and processes of the organization of a particular individual, but also the individually characteristic manner in which they are coordinated or related to one another to produce the particular organization encountered at any given instant. The organization of a living system considered with reference to the probability of its persistence, or to its resistance, is frequently referred to by pathologists and medical men as the constitution of the organism. If susceptibility to pathic events is limited to specific ones, the state is by some designated as disposition or predisposition (Lenz, 18). Others apply the term disposition to all factors of the organization of an organism which condition abnormal susceptibility to pathic events, and limit the term constitution to hereditary dispositions. This usage is undesirable, however, because the genetic constitution of an organism directly or indirectly plays a rôle in practically every disposition.

Figure 1 represents that not only the organization or constitution of the plant, *O*, and the immediate effect of the external environment simultaneously enter into practically every pathic event, but also that the constitution of the plant at any moment is the resultant of a complicated interplay of its original organization (both genetic and non-genetic), and of the whole procession of external environmental factors that have impinged upon it. At times the effect of the genetic constitution, as in albinism of *Zea mays* and in other pathic events conditioned by lethal genes, is so overwhelming that external factors may be practically ignored. The so-called hereditary pathic events are of this type. In most instances the genetic constitution plays an important complementary rôle

in that it conditions the reaction to other factors, both internal and external. In some cases the external environment affects the plant catastrophically so that the constitution may be practically ignored. In most instances, both the constitution of the plant and external factors play a complementary rôle. The diagrams used here are an adaptation of a type used by my colleague, Professor Sewall Wright, in analyzing problems of physiological genetics. They are a much more suitable medium for expression of multi-directional and simultaneous relations than the spoken or written word, which perforce must deal with factors *seriatim* just as experiment must change only one factor at a time (198).

Precise analysis of any complex pathic event, such as a pathic malformation or an infectious disease, sooner or later compels one to deal with a part (part-event) of the total or complex event at a definite moment and at a definite place, which usually is one of the cells of the organism in which the pathic event occurs. By arranging the part-events in their proper sequence a series of antecedents and consequents is

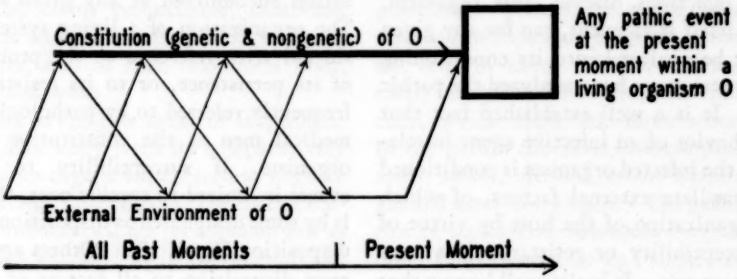

Constitution (genetic & nongenetic) of O
Any pathic event at the present moment within a living organism O
External Environment of O
All Past Moments
Present Moment

FIG. 1. SHOWING THE RELATIONS OF THE ANTECEDENTS, CONSTITUTION AND EXTERNAL ENVIRONMENT, TO EACH OTHER, AND TO THEIR CONSEQUENT, THE PATHIC EVENT OR PATHIC PART-EVENT

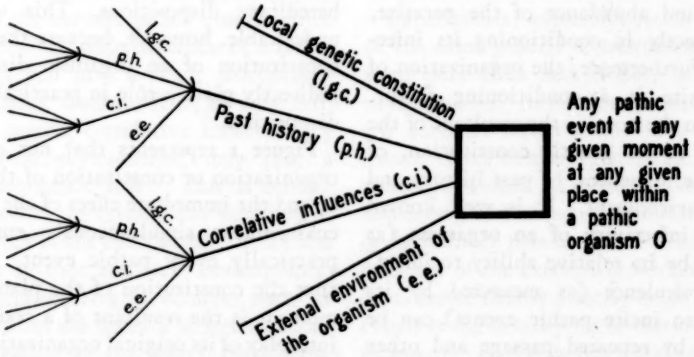

l.g.c.
p.h.
c.i.
e.e.
Local genetic constitution (l.g.c.)
Past history (p.h.)
Correlative influences (c.i.)
External environment of the organism (e.e.)
Any pathic event at any given moment at any given place within a pathic organism O

FIG. 2. DIAGRAM OF THE FACTORS WHICH ARE ANTECEDENT TO ("CONDITION" AND/OR "INCITE") PATHIC EVENTS IN ORGANISMS

For further explanation see text

established which gives a complete etiological description or explanation of the complex or total event.

That every part-event in such an analysis arises out of a complex of antecedents is shown in figure 2, which is a schematic representation of all possible factors or effects which condition and/or incite pathic events in developing and mature organisms. The square to the right represents any given pathic event or part-event at a given time and place within a given organism, *O*, and the four lines converging upon the place represent the effects of: (1) its local genetic constitution, *l.g.c.*, both chromosomal and cytoplasmic; (2) its past history, *p.h.*, manifest mainly in the non-genetic cytoplasm and its products; (3) correlative influences, *c.i.*, of other places within the organism; and (4) the immediate external environment of the organism, *e.e.*, including foreign organisms. The diagram represents that the factors of past history and of correlative effects may each be resolved into the four factors just enumerated. At certain instants only one of the four factors may play the decisive inciting rôle, the others playing a rôle as present or as past conditioning factors. In the latter case, their effect is represented by the past history factor which includes such factors as the physical and chemical constitution of the place at the moment under consideration. The diagram also represents that while any of these factors taken alone may not be pathogenic, certain combinations of them often are. Finally the diagram shows that when we speak of "the cause" of some pathic event we are merely designating one noticeable factor of a continuous process, or one factor which has been dealt with singly in experiment, as "cause" and one which follows it, as "effect."

The temporal and spatial relations of a pathic part-event at a given time and place, *A*, in a multicellular organism, *O*, are represented in figure 3. The direction of time's flow is indicated by the arrow at the base of the diagram, and the total time is divided into

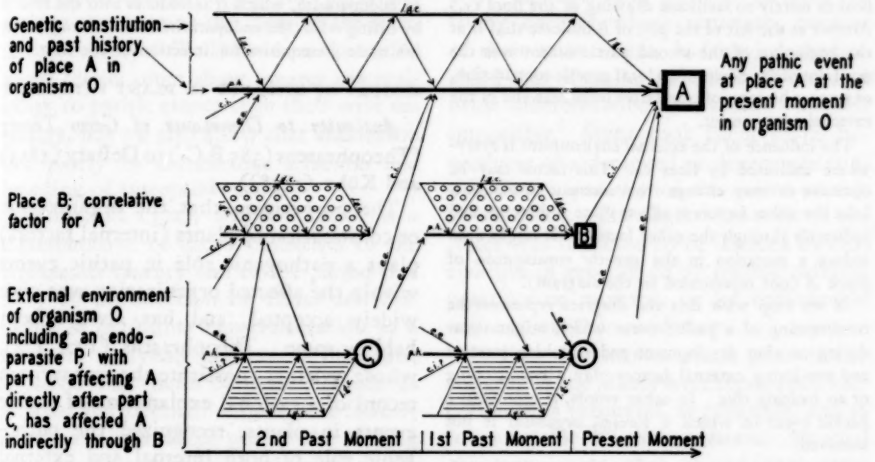

Genetic constitution and past history of place A in organism O
Any pathic event at place A at the present moment in organism O
A
Place B; correlative factor for A
B
External environment of organism O including an endo-parasite P, with part C affecting A directly after part C, has affected A indirectly through B
C
C
2nd Past Moment
1st Past Moment
Present Moment

FIG. 3. SHOWING THE SPATIO-TEMPORAL RELATIONS OF THE ANTECEDENTS OF A PATHIC EVENT OR PATHIC PART-EVENT TO EACH OTHER AND TO THEIR CONSEQUENT, AT A GIVEN TIME AND PLACE IN A MULTICELLULAR ORGANISM

For further explanation see text

present, 1st past, 2nd past, and all other past moments. The heavy line, *p.h.*, of the uppermost truss, represents the past history of place *A* in which we are specially interested at the present moment. Connection of line *p.h.* by arrows with the line *l.g.c.*, which represents the local genetic constitution of place *A*, indicates that the cytoplasm and genetic constituents of place *A* were in constant interplay, so that the latter (*l.g.c.*) conditions what goes on in the former (*p.h.*), while what has gone on in the former (*p.h.*) in turn conditioned the influence of the local genetic constitution upon the cytoplasm of place *A* at each moment. Similar relations between the cytoplasm (*p.h.*) and the genetic constitution

(*l.g.c.*) are indicated by the trusses for every other place represented in the diagram, i.e., for place *B*, which is any other place in organism *O*, and for *C* and C_1, which are places within the endoparasite *P*, which though within organism *O*, is considered part of the external environment of the organism *O*. Arrows *l.g.c.*, *p.b.*, *c.i.* and *e.e.*, indicate that place *A* at the beginning of the second past moment was the resultant of the effects of these factors during all past moments.

The correlative influence of place *B* upon *A* is indicated by the middle set of trusses of the diagram. Its effect upon place *A* is indicated by arrows drawn to *p.b.* of *A* at the close of the second past moment, and to *A* in the present moment. (The truss is broken merely to facilitate drawing of the lines *e.e.*) Arrows at the left of the *p.b.* of *B* indicate that *B* at the beginning of the second past moment was the resultant of the effects of its local genetic constitution, of its past history, of correlative influences and of the external environment.

The influence of the external environment is everywhere indicated by lines *e.e.* This factor may be constant or may change from moment to moment. Like the other factors it affects place A directly, and indirectly through the other factors. It might even induce a mutation in the genetic constitution of place *A* (not represented in the diagram).

If we stop with this the diagram represents the conditioning of a pathic event which might occur during or after development and in which internal and non-living external factors play a conditioning or an inciting rôle. In other words, it represents a pathic event in which a foreign organism is not involved.

If we choose to represent a pathic event in place *A*, incited by a parasite, specifically an endo-parasite (an infectious disease, for example) we need only add a third set of trusses in the region from which the external environmental factor lines originate. We will assume that the parasite enters the organism *O* at the beginning of the 2nd past moment and that this individual or another, after being in direct interplay with place *B* or any place other than *A*, directly affects place *A* in the present moment, as part of its external environment. Places *C* and C_1 of the parasite *P* are resultants of an interplay of their past history, local genetic constitution, of correlative influences within the parasite, if there are any, and of the external environment of the parasite, the most important of which is the host *O*. The interplay between places *C* and C_1 and *B* during the two past moments is indicated by arrows extending from *C* and C_1 to *p.b.* of *B*, and from *p.b.* of *B* to *p.b.* of *C* and C_1.

The diagram shows that previous to the moment under analysis the parasite affected a place (or places) other than *A*, and that these places affected it. The effects may consist of increased or decreased invasiveness or virulence of the parasite at the time it reaches place *A*, and through correlative influence of place *B* upon *A*, of an increased or decreased susceptibility of place *A*. For simplicity's sake, the correlative influence of different places of the parasite upon the one under consideration is omitted. Such influences would exist only in a multicellular parasite. That the parasite and the affected places are under the influence of common external factors also is indicated by lines *e.e.*

The same type of diagram may be used to represent cases in which an insect vector is the obligate host of an endoparasite, which it introduces into the host *O* by biting when the endoparasite is in the stage of its life cycle prerequisite for infection of the host.

HISTORY OF ETIOLOGY IN PLANT PATHOLOGY

Antiquity to Dominance of Germ Theory (Theophrastus (385 B.C.) to DeBary (1853) and Kühn (1858)).

The proposition that the organization or constitution of plants (internal factors) plays a pathogenic rôle in pathic events within the affected organization was once widely accepted, and has always been held by some. Theophrastus (170, 171), whose writings constitute the first written record of a rational explanation of pathic events in plants, recognized the pathogenic rôle of both internal and external factors including in the latter, flowering plants, fleshy fungi, insects, and other animals that live on plants. Disturbances in the constitution of the plant he thought were the material basis for diseases, and the constitution of the plant was supposed by him to be conditioned to a great extent by the nature and proportion of the food the plant took in from its substrate. His etiological pathology, so far as disease is concerned, apparently was influenced by the improper mixture (discrasy) idea of the humoral pathology of Hippocrates. Since observation had indicated that culture affected the constitution of plants markedly he assumed that it conditioned

diseases directly and indirectly. He thought that the lack of reports of diseases in wild plants might be due to an actual absence of diseases in wild plants, because their constitution had not been subjected to the weakening influence of cultural practises, especially watering and fertilizing. These ideas of Theophrastus were handed down with slight elaboration and had a profound influence for more than two thousand years. They even played an important rôle when the germ theory of disease began to command serious attention at the close of the nineteenth century. We know now that cultivated plants often show greater susceptibility to pathic events than their wild ancestors, but we also know that the reason lies partly in unconscious selection and breeding of susceptible forms.

Tournefort (172), whom I select as a representative of the systematists of the eighteenth century and of the period just prior to introduction by Hales and Duhamel of deliberate experimentation as a means of analyzing phenomena in plants, classified the causes of plant diseases as internal and external. Among the former, nutritive disturbances played the leading rôle; among the latter he placed the nonliving external factors, as well as parasitic plants and insects. As the century passed the influences of de Saussure and Ingenhousz began to focus attention upon the processes within the plant body and upon the material income and outgo of plants. These discoveries were taken to support the ancient proposition that improper mixtures of the constituents of the plant condition disease. Disturbances in the material income and outgo of plants were assumed to lead to improper mixtures of the constituents of the plant, especially the sap, and thus pave the way for the metamorphosis of healthy organs and plants into diseased ones. The physiological and pathological ideas of the late eighteenth and early nineteenth centuries did not advance much beyond this because botanists again had become engrossed in morphological problems. Development of the achromatic lens (1827) had opened new vistas of structure.

Following introduction of the lens into Europe and its improvement as a scientific instrument in the late seventeenth and in the early nineteenth centuries, investigators observed that many plant diseases were characterized by the presence of living entities whose structure, because of their minute size, had eluded the unaided eyes of earlier investigators and had made differentiation into distinct entities impossible. Some took them to be by-products of a deranged or degenerate constitution, especially of the juices or sap of the plant. The degenerate state of the juices was supposed to be induced by cultivation in general, or by specific cultural practices or mishaps, or by unfavorable soil and weather conditions.

In time the entities which grew on the surfaces of the plants were recognized successively as epiphytes, as parasites, and as fungi capable of inciting disease. By 1850 the proposition that these fungi were capable of self-propagation and characterized by genetic continuity in which germinating spores played a rôle had been advanced by some investigators.

The entities which seemed to come from within the plant, especially the exanthemata such as the smuts, rusts, and white rust, as well as other fungi, were considered something quite different up to about the middle of the nineteenth century. They, too, were in time recognized as distinct living things but were designated as entophytes in contrast to the epiphytes. Unger (176) in 1833 did not consider them true fungi but distinct disease organisms which arose in the diseased plant and

simulated fungi. Meyen (127), on the basis of a few experiments of his own and evaluation of the experiments and conclusions of others, agreed with Unger that these organisms were not self-propagative. By 1850 some writers in plant pathology like Meyen, in harmony with some of the mycologists, interpreted these organisms to be fungi which arose spontaneously by transformation of the sick sap or tissues of the affected plant.

Neither Unger nor Meyen accepted the conclusions of previous experimenters who had demonstrated the infectious nature of these entophytes. Meyen in the main shared Unger's views but, in discussing Gleichen's experiments of 1772, which demonstrated that grain treated with smut spores yielded more smutted plants than grain not so treated, assumed that an infective principle passed from the spores into the soil and from there into the seed and seedlings, thus predisposing the sap of the plant to the smut disease. Both Unger and Meyen rejected Knight's conclusions of 1805, based on infection experiments of wheat with spores from the rust on barberry.

DeBary (8) in 1853, on the basis of anatomical studies of tissues bearing smuts, rusts and similar fungi, demonstrated the presence of structures which he interpreted to be the vegetative part or mycelium of the organisms associated with the exanthemata. On the basis of his own anatomical studies, the observations and experiments of others on the germination of fungus spores, and of the "sporidia" of the entophytes, he concluded that these organisms were true fungi, with genetic continuity, and that they were parasites capable of invading the host plant from without by their germ tubes which grew out of the spores. His zeal in advancing the proposition of genetic continuity and individuality of these organisms led him to reject the proposition that the spores of the barberry rust could infect the wheat. DeBary's experimental work which proved most of his propositions of 1853 and put to rout the idea of spontaneous generation so far as fungi are concerned did not begin to appear until 1861.

During the earlier stages of these developments the proposition that the constitution of the pathic plant plays a rôle in disease was part of physiological pathological theory. The treatises on plant pathology, even when separate from those on physiology or other phases of botany, had a distinct physiological complexion. Thus Wiegman (194) in 1839 classified pathic events in plants as diseases of (1) the nutritive system, (2) the respiratory system, (3) the reproductive system.

Unger (176) assumed that the beginning of the diseases characterized by exanthemata lay in an improper mixture (discrasia) of the sap of the vascular system of the plant. Various factors, both internal and external, were supposed to induce this condition and evidence for this view was sought in the work of Saussure, Ingenhousz, and Hales. Certain varieties, species and young plants, especially the cultivated ones, were reported to be more susceptible than others. Under atmospheric conditions which limited respiration and transpiration the diseased sap was supposed to accumulate in the succulent tissues, and in time to metamorphose into a matrix out of which the cells of the exanthemata or entophytes arose. He carried on anatomical studies and in his sections apparently mistook the mycelium in the tissues for the diseased sap matrix. He did not use the various reagents which aided DeBary in his correct interpretations. The ideas of Unger were in harmony with the ideas current at that time about metamorphosis, epigenesis and the cell theory of Schleiden

and Schwann as well as with the disease theory of DeRokitansky (153), the human pathologist. The anatomical studies of the best investigators of the day indicated that free cell formation could occur.

Meyen (127) assumed that improper mixtures of the nutritive substances of plants, induced by internal and external factors, constituted the diseased state in the exanthemata and many other diseases, and that the entophytes arose either within diseased cells or by direct transformation of cells, or from the exudations of diseased cells.

Both Unger and Meyen recognized a causal complex in these diseases and the rôle of predisposing factors in preparing the way for development of the foreign organisms. They stressed the rôle of atmospheric influences and recognized both generic and individual differences in susceptibility to disease.

Dominance of the Germ Theory, 1853–1874—(DeBary (1853) and Kühn (1858) to Sorauer (1874)).

DeBary in the paper of 1853 which turned the course of plant pathology from a broad biological channel into a mycological-parasitological one, advanced the proposition that entophytes, which he considered to be parasitic fungi with a genetic continuity, are the *cause* rather than the *result* of disease. He interpreted everything known about infectious diseases in terms of the parasite. Observations that non-living external environmental factors play a rôle in these diseases were interpreted to mean that these factors affect the abundance, dissemination and germination of the spores of the parasites. The observation that some individuals, varieties, species, genera and families are infected and others not, he explained by assuming that specialization existed on the part of parasites. While he thought that the smuts were "hereditary diseases" and that perennial mycelia might be involved, he favored the assumption that new infections occurred annually. While he was not interested in the pathology of the hosts he stated clearly that disturbed cellular functions followed upon invasion by the parasite, and recognized that not all diseases are due to stimuli by parasites. In so far as he paid any attention to control measures he urged eradication of the parasite or prevention of dissemination of its spores. He designated the fungus as "*Ursache*," and there is nothing in his paper to indicate that he recognized a causal complex. He was the first of the germists, and was soon outdone by those who not only limited the etiological propositions about infectious diseases to the parasite, but also limited the field of pathology to diseases incited by parasites.

The first experimental proof of DeBary's propositions appeared in 1858 with publication of Kühn's (106) *Krankheiten der Kulturgewächse, ihre Ursachen, und ihre Verhütung*. Kühn was a practical agriculturist with botanical training and his book had a great influence on the course of development of practical and theoretical plant pathology. He recognized that infectious diseases do not exhaust the field of pathology, but devoted only 20 out of 310 pages to non-parasitic diseases. Under Liebig's influence, he stressed the rôle of the soil in the pathogenesis of non-parasitic diseases. He did not recognize the rôle of internal factors excepting reluctantly in connection with diseases which we now know are incited by bacteria. The absence of fungi in the lesions of these diseases perplexed him and he wrote, "These [diseases] are based on an abnormal change of cell content by virtue of which the composition of the cell membrane is also changed and destruction of the involved tissue or of the entire plant may result." His etiological conceptions

about infectious diseases were identical with those of DeBary.

With the publication of DeBary's researches on the rust and smut fungi, begun in 1863, and on the *Phytophthora* disease of the potato in 1861, a tremendous interest developed in the structures, life histories, and biology of fungi, which expressed itself in research in the botanical institutes then being created. It is not surprising that under the circumstances a naïve germ or fungus theory of disease developed. Had it not been demonstrated that the supposed exanthemata or by-products of a deranged constitution of both animals and plants were discrete entities with a life cycle of their own? Were not both cultivated and non-cultivated plants affected? Was it not possible to inoculate plants with these organisms and produce disease? Now at last the opportunity had come to establish "*the*" cause of a disease as clearly as the physical sciences were thought to establish causes. Moreover this cause could be isolated, cultivated, and experimentally inoculated into plants. When similar successes were met in the study of the rôle of bacteria in animal diseases, disease seemed to lose some of its mystery. At last something definite, tangible and material had been found that *caused* disease; something that enabled one to put pathic phenomena on a verifiable and predictable basis. This was a welcome escape from the unsatisfactory, refractory and even mystical internal factors which were included in the concept of "constitution." Metaphysically and methodologically, therefore, there were good reasons for the clean sweep of the germ theory. Like other great concepts of science, it was taken too literally and it was too successful in the hands of zealots. This is partly due to the fallacies of the generally accepted viewpoint about science and causality. Chief among these, as expressed by Jennings (96) is

> the fallacy of attributing to one cause what is due to many causes.....Everywhere there is search for "the" cause of this or that phenomenon; the investigator is not content until he has found "it." Yet natural phenomena—and most emphatically is this true of biological phenomena—merely arise out of the complex situation in which they occur. Many elements of that situation affect them; and all that experimental science can do is to determine what difference is made by altering one or more of these elements; none is "the" cause to the exclusion of the others.....When he [the investigator] has found "the" cause, it follows that other proposed causes are not concerned in the matter, can be left out of account, must be rejected. From positive observations that one factor is at work, the conclusion is drawn, that others are not; a vicious fallacy.

These fallacies have their basis in part in the attitude of the human mind which makes science possible, the craving for simplicity, for classification, for finding the regular, the repeatable, the "questing for certainty," and the necessity, in experiment, of altering only one factor at one time. These lead to "the fallacy of concluding that because one factor plays a rôle, another does not; the fallacy of drawing negative conclusions from positive observations." Logically, therefore, if parasites are "the" cause of diseases nothing else is. This is, I believe, in part the background for the all too-successful sweep of the germ theory.

It is a significant fact in the histories of plant and human pathology that Kühn's book, which stressed "parasitic diseases" and the parasite as the sole pathogenic factor, and Virchow's *Die Cellularpathologie in ihrer Begründung auf physiologische und pathologische Gewebelehre* were published the same year, 1858. While the germ theory greatly influenced human pathology, the long history of anatomical and physiological emphasis, and DeRokitansky's

(153) and Virchow's influence asserted themselves so strongly that human pathology did not incorporate or become a part of bacteriology to the extent that plant pathology incorporated or became part of mycology. Human pathology never quite forgot that the pathic organism is the central theme of pathology, and hence has stressed the study of pathic structures and processes rather than the biology of parasites. However, it should not be forgotten that DeBary in 1853 (8) clearly formulated the proposition that cellular disturbances are the material basis of infectious diseases of plants.

Revival of Concepts of Environmentally and Internally Conditioned Susceptibility (Sorauer (1874)).

While the germ theory was getting under way two trends of etiological thought developed in Germany among the plant pathologists; that of the germists, who considered the parasite the sole cause of infectious diseases; and that of a larger group designated by Whetzel (193) as the predispositionists, who supported the proposition that external non-living factors play a pathogenic rôle by predisposing the host to attack by the parasite. The latter group should really be called environmental predispositionists. Their first spokesman was Sorauer, who in his first edition of 1874 (166) stressed the rôle of external non-living factors in infectious and non-infectious diseases. He also discussed vaguely, as did Hallier (84), Frank, and Hartig, the rôle of what is now known to be hereditarily conditioned susceptibility. Sorauer (166) staunchly sponsored the proposition that the constitution and disposition of the plant play a rôle in pathogenesis. By 1884 even DeBary recognized the rôle of predisposition in the genesis of infectious diseases. Sorauer's formulation of the concepts "constitution," "internal factors," and "hereditary diseases" was so vague, however, that they did not establish themselves in etiological plant pathology. The situation at the turn of the nineteenth century was summarized by Ward (183), who, writing about 1900, classified by proximal causes of plant diseases as: 1) external agencies and 2) internal agencies, and stated,

> While such a classification may serve its purpose as a sort of index, it must be confessed that the limits of its usefulness are soon reached. In the first place, the so-called "internal causes" of disease is probably a mere phrase covering our ignorance of the factors at work, and although a certain convenience attaches to the distinction between cases where tender breeds of plants apparently exhibit internal predisposition to suffer more readily than others from parasites, low temperatures, excessive growth, etc.,—the mystery involved in the phrase "internal causes" only exists until we find what action of the living or non-living environment of the essential mechanism of the plant has upset its equilibrium.

Rise of Analysis of Internal Factors (*Developmental Physiology*, Sachs (1870); *Genetics*, Mendel (1866)).

In the meantime two types of investigation had gotten under way which were to put the concepts of constitution, internal factors, and hereditary diseases on an objective and scientifically fruitful basis. Sachs, Pfeffer and Goebel, as well as Roux and others interested in problems of developmental physiology and morphology, advanced the proposition that internal factors play a rôle in development of form, and amassed a body of data whose general significance for developmental physiology of plants has been summarized by Pfeiffer (141) and by Goebel (79), and for plant pathology by Küster (107). The work of the developmental physiologists or morphologists has clearly established that internal factors such as correlative

and past history influences play an etiological rôle in all types of events in plants.

Analysis of the rôle of internal factors in the processes and structure of plants and animals received its greatest impetus with the rediscovery of Mendel's Laws in 1901. While the germ theory was occupying the thought of pathologists, Mendel from 1866 to 1870 (126) was trying to win the assent of his botanical colleague, Nägeli, to certain propositions about inheritance in plants. If these had been accepted at that time they might have opened the way for a precise and objective analysis of the constitution of the plant, as well as of heredity, variation and evolution, and might have become a part of the etiological propositions of plant pathology during its formative period.

The rediscovery of Mendel's Laws (126) inaugurated the present era of intensive analysis of the genetic constitution of organisms and of the rôle of internal factors in development and behavior of the individual and species. The technique of genetics has made possible such an analysis of the hereditary constituents of the organism that the "constitution" of organisms, which once formed the basis for the vaguest and most mythical biological concepts, has furnished the most objectively and quantitatively established data. Synthesis of the analytical phases of genetics and developmental physiology provides an additional tool for analysis of the constitution or organization of living things, and return to the proposition that both internal and external factors play a pathogenic rôle in pathic events.

The data, concepts and propositions of synthetic genetics should therefore come to play an important rôle in the future development of etiological pathology, just as the subject matter of pathology has enriched genetics. Possibly something can be gained by a union of these sciences for, as stated by Poincaré (142), "we should find the greatest advances in the past have happened when two of these sciences have united, when we have become conscious of the similarity of their form despite the differences of their matter, when they have so modelled themselves upon each other that each could profit by the other's conquests."

The gains for pathology in recognizing that potentially pathogenic factors exist within the organization of the plant are: 1) Pathology need no longer neglect the field of hereditarily conditioned pathic events. 2) A more rational conception and presentation of constitution, predisposition, susceptibility, infectivity and virulence is possible. 3) There would be more general recognition that when one speaks of "*the cause*" of a pathic event relations are represented too simply, and that the phrase really means that a given event is not the effect of a single cause "but the resultant of causes infinitely numerous . . . the consequence of the state of the universe a moment before." The concept "the cause" would give way to the concept "a complex of causes." This will lead to more penetrating analysis of pathic events and an integration of apparently unrelated data. It will also have practical results in commerce and in the courts. There have been instances in which the simple notion of cause has worked for social injustice. In certain civil suits involving claims arising out of losses to fruits and vegetables in transit and storage, it has been contended that a fungus was the cause of the pathic events responsible for the losses, and that, since the fungus could be traced to the field in which the crop was grown, the grower should be held solely responsible. As a matter of fact, the losses might have been avoided if temperature conditions had been provided which are known to

keep the fungus in check, though present. This is an excellent illustration of the monetary significance of the metaphysical implications of the idea of cause. 4) A basis may be established for classifying control measures into those directed at the pathic organism itself, and those directed against the immediate factors of the environment, non-living or living, as direct or indirect pathogenic factors. 5) Finally, it will bring a more thorough recognition that pathic events and states are not abnormal in the statistical sense, but that the individual usually persists as long as it does by virtue of new adjustments to pathic events and that the species circumvents death as a species largely by virtue of a tremendous fecundity.

This paper is an expansion of one presented before the Biology Club of the University of Chicago, June 6, 1930. At that time it was illustrated with living material. In the course of elaboration of this paper, I have profited greatly by exchange of ideas with my colleagues, Professors Benjamin Willier and Sewall Wright, who have represented developmental physiology and physiological genetics respectively in our discussions.

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THE INFLUENCE OF TEMPERATURE ON THE LIFE PROCESSES AND DEATH OF BACTERIA

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THE propagation of all bacterial life is necessarily limited by temperatures which may affect the physiological functions of catabolism and metabolism of such unicellular organisms. The survival or death of bacteria submitted to high temperatures is likewise dependent on the changes induced within the organisms. Since the chemical constitution of the cells consists primarily of protein, dissolved salts and water, any influence which may affect one or more of these components controls the growth and the survival of the species. The temperature range of growth and survival of bacteria must therefore lie between the temperature above which their chemical constitution is not permanently altered by freezing and below which their proteins are not precipitated or denatured by heat.

OPTIMUM TEMPERATURE FOR GROWTH

Bacteria have been divided roughly into three classes, depending upon their optimum temperature requirements for growth —namely, psychrophiles, mesophiles and thermophiles. Psychrophiles are organisms which grow best at 10° to 20° to 30°C. They do not multiply at 40°C. Mesophiles thrive best between 37°C. and 40°C. They do not multiply at 55°C., and are usually dormant at 20°C. Thermophiles have an optimum temperature above 55°C., and do not grow at 40°C. The lines of division are not strictly marked, however, and there is much overlapping of the classes. Some species have a very wide range of growth, while in others the optimum temperature range is limited to four or five degrees. The relation of nutritional requirements to optimum temperature requirements is also a complicating factor, for although the data are meager, it has been demonstrated in a few instances at least that the optimum temperature for growth may be influenced strongly by the amounts and kinds of available nutritional elements present (Buchanan and Fulmer, 1930).

Because of the greater interest in this class of organisms the mesophiles have been the center of most of the detailed study as to the factors which influence the multiplication and death of bacteria. The disease-producing organisms and the majority of the putrefactive and fermentative bacteria belong to this group. The growth and death curves of many of the species have been studied in detail. Their chemical constitutions and enzymatic actions have been the subjects of many investigations. But the information accumulated up to the present time is meager compared with our vast ignorance of their inherent nature. What factors are necessary for the growth of each species? Wherein does one species differ from another in chemical and enzymatic nature? What rearrangement or alteration of the molecules induces death? Why is one species parasitic while another closely allied species is saprophytic? These are

typical questions which occur to the student of the physiology of life and death of bacteria.

Even less is known of the fundamental characteristics of psychrophiles and thermophiles. The psychrophilic bacteria are commonly found in water at low temperatures, and they predominate in soil during the winter months. They have been found to multiply to such an extent in milk or butter kept at low temperatures that a disagreeable flavor is imparted to the product. Marine psychrophiles have also been isolated from spoiled iced fish stored at −5°C., where a bacteriological examination showed them to be responsible for the proteolytic processes in the fish muscle (Sanborn, 1930). Likewise beef in cold storage has been found to develop disagreeable odors and a slimy coating due to the presence of microorganisms capable of growing at a temperature several degrees above freezing (Prescott, Hale and White, 1931). These organisms are characterized by a slower rate of growth and a higher water content than other bacteria. Little is known as to their chemical constitution and the quality and type of their enzymes (Loehnis and Fred, 1923).

Thermophiles are found in hot water springs, in feces, in decaying vegetable matter and in the soil in tropical climates. Because of their peculiar resistance to high temperatures they have likewise been isolated from pasteurized milk and canned foods. We have here a group of organisms which not only live but also require for multiplication a temperature which in many instances destroys other bacteria. These bacteria are known to have a lower water content than other species. It was also demonstrated by Feirer (1927) that they produce certain enzymes, for example diastase and catalase, at temperatures where other enzymes are destroyed. From the results of a more recent investigation, it would appear, however, that enzymes of mesophilic and thermophilic bacteria which produce similar reactions are more or less type specific. For instance, five known thermophiles, cultivated at minimum temperatures supporting their growth, were studied by Casman and Rettger (1931) with respect to the heat lability of their enzymes. All exhibited destruction of the succino-dehydrogenase after heating at a temperature approximating the maximum temperature of growth. Oxidase, and, in two organisms, catalase showed a similar but not so sharply defined relationship.

GROWTH CURVES

The growth curves of pure cultures of all bacteria assume essentially the same shape. According to Winslow (1928) the ideal curve is divided into five phases, as represented in Figure 1. In a freshly seeded bacterial culture there is a lag or adjustment phase which, according to Chesney (1916) and others, is an expression of the injury received by the cells in their previous

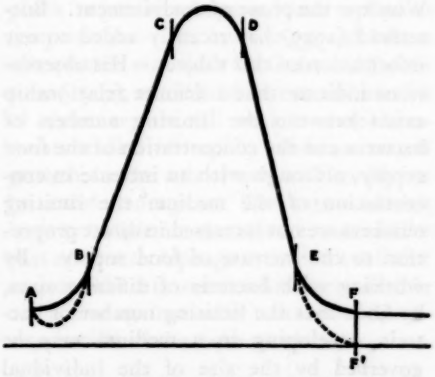

FIG. 1. IDEAL CURVE OF A BACTERIAL POPULATION CYCLE

Ordinates = numbers; abscissae = elapsed time. A–B. Phase of adjustment; B–C. Phase of increase; C–D. Phase of crisis; D–E. Phase of decrease; E–F. Phase of readjustment. (Winslow, 1928.)

environment, while Sherman and Albus (1924) believe that a definite interval of time is required for the old cells to undergo a biologic rejuvenescence which fits them for reproduction. Multiplication then begins and gradually proceeds logarithmically for a time. This is followed by a period of negative acceleration, according to Buchanan (1928), until the peak of the growth has been reached. The curve then flattens and the bacterial population remains stationary, so far as numbers are concerned, for a definite period, after which the organisms begin to die off more rapidly than the new cells are formed and the curve descends logarithmically, then more slowly until either the food supply becomes exhausted or the accumulation of toxic products of metabolism becomes so great that the organisms pass into a dormant or resting stage. This is called by Winslow the phase of readjustment. Butterfield (1929) has recently added to our information on this subject. His observations indicate that a definite relationship exists between the limiting numbers of bacteria and the concentration of the food supply, although with an increase in concentration of the medium the limiting numbers are not increased in direct proportion to the increase of food supply. By working with bacteria of different sizes, he finds that the limiting number of bacteria developing in a medium may be governed by the size of the individual cells, and that the extent of depletion of the dissolved oxygen may be proportional to the total volume of the cells produced, rather than to the actual number of cells. There also seems to exist a close agreement between the amount of food present and the volumes of different organisms produced.

The complete cycle may be a long one lasting for days or weeks, as in the case of psychrophilic organisms; it may require a moderate time interval, with the lag phase requiring only four to six hours, as in the case of many mesophilic organisms, or else the peak of the curve may be attained in five or six hours, as has been shown in certain of the thermophiles. The reason for this is not altogether clear but since it is known that heat increases the speed of chemical reactions we might expect the life cycle of thermophilic bacteria to be shorter than that of other species whose optimum temperatures are much lower. It is also suggested that the water content of the bacteria may play an important part. Psychrophilic organisms and certain of the mesophiles contain a large amount of moisture, which may dilute the enzymes within the bacterial protoplasm, whereas in thermophilic bacteria there is a lower water content and therefore less dilution.

With this brief review of the influence of temperature on the growth of bacterial populations, we may now proceed to a more detailed survey of the destructive action of heat with a consideration finally of certain practical problems which have arisen from the question of heat resistance in the pasteurization of milk and in the canning industry.

CAUSES OF DEATH OF BACTERIA BY HEAT

A discussion of the causes of the death of bacteria due to heat may be divided into four portions: dry heat and moist heat at low and at high temperatures.

At low temperatures dry heat causes the formation of oxidation products which destroy bacteria (Paul, Birstein and Reusz, 1910). At high temperatures two processes may occur. Coagulation of the proteins takes place in most instances. Rubner (1899), however, has introduced experimental evidence to show that death of bacteria by dry heat may be due to a scorching or carbonization of the outside

of the organism, which interferes with its nutritive processes.

The complete destruction of bacteria by heat was first studied by Koch (1881), who, as a result of a series of carefully planned experiments, laid down the fundamental principles of heat sterilization, which are in use today. In his early work on anthrax he had found that hot air at a temperature of 140°C. for three hours was required to destroy the spores, whereas they were killed in a few minutes by boiling. In investigating the reason for this, he was able to show by the use of maximum thermometers placed at various depths in balls of wool and linen that a longer time was required to bring the thermometers to a temperature destructive to bacterial spores when dry heat was used than when streaming steam was employed. He demonstrated further that steam placed under pressure elevated the temperature readings of the thermometers and effected sterilization even more rapidly than streaming steam. His explanation lay very simply in the more rapid penetration of the heat with streaming steam or steam under pressure. Later Hellmich (according to Lewith, 1890) obtained heat-coagulable albumin from cell contents of bacteria and in 1890 Lewith was able to show that the temperature of coagulation of pure albumin is inversely proportional to its water content, and hence that hot water or steam owes its efficacious properties to the dilution of the protein material along with the heat. This theory has been accepted with certain qualifications, and offers an explanation for the highly heat-resistant properties of bacterial spores in contradistinction to vegetative cells, since the water content of spores is known to be exceedingly low. It has been suggested also that moist heat produces decomposition products—such as carbonic acid, ammonia and hydrogen sulphide—in the organisms which aid in the destruction. These products have actually been derived from heating egg albumin and wool (Ford, 1927). The oxygen molecule of the water is also thought to play a significant part.

At low temperatures, however, moist heat affects the organisms in an entirely different way. Bacterial cultures submitted to temperatures just slightly above their optimum temperature become sterile, due to a speeding up in the physiological activity of the organisms with a subsequent rapid accumulation of metabolic products which in turn destroy the organisms. This is not only true of mesophilic organisms exposed to temperature two or three degrees above their optimum but also of thermophiles. Indeed, Tanner and Wallace (1925) have shown that thermophiles even at their optimum growth temperature die off more rapidly than mesophilic organisms kept at their optimum temperatures. An interesting and recent observation was made by Gohs and Jacobsohn (1927), who found that certain strains of dysentery bacilli which were heat-resistant died off rapidly after an initial growth in the subculture, and could not be transferred again. The nature of this lytic principle in the culture was unlike that of a bacteriophage or an extra-cellular enzyme, since it was neither transmissible nor active against other cultures of dysentery bacilli of the same strain.

THERMAL DEATH POINTS OF PATHOGENIC BACTERIA

The thermal death points of pathogenic bacteria have been extensively studied due to the practical importance attached to such determinations in hygiene and medicine. The accuracy of such determinations, however, has been difficult to establish because of the many new factors which have been introduced from time to time. The early investigators pointed out

TABLE 1

Effect of high temperatures on death of certain well-known bacteria

INVESTIGATOR	RESULTS
1. *Bacillus coli*	
Loeffler, 1886	24 hr. neutral broth culture of *B. coli communis* in Erlenmeyer flask was destroyed in 10 min. at 60°C.
van Geuns, 1889	Organisms destroyed at 62°–63° in 1 min. and at 59°C. in 5 min. when heated in sealed capillary tubes.
Hesse, 1900	Bacilli killed in 15–20 min. at 60°C. when heated in milk without formation of scum.
Escherich and Pfaundler, 1903	Work of Kitasato, Weisser, Chautemesse and Widal quoted. Thermal death point at 60°–61°C. for 5 to 15 min. exposure. Work of Fränkel quoted. Thermal death point at 55°–60° for 120 min.
De Jong and De Graef (quoted by Rullman, 1914)	Of 11 strains of bacteria isolated from pasteurized milk, 7 were *B. coli* which were viable after heating in milk and in bouillon for 30 min. at 65°–67°C. Some survived for 35 min. at 73°–75°C. in fresh milk.
Shippen, 1915	Of 31 cultures isolated from pasteurized milk, 11 survived 60°C. for 15 min. Three of these survived 62°C. for 30 min.; 3 others survived 63°C. for 30 min. One strain had a thermal death point of 68°C. for 15 min. and 65°C. for 30 min. None of 13 strains isolated from raw milk survived 60°C. for 15 min. Technic: peptone water cultures heated in sealed capillary tubes.
Zeeler, 1927 (quoted by Reichel)	Holder pasteurization methods found to destroy members of colon group of bacteria.
Richter & Schäffer, 1927 (quoted by Reichel)	*B. coli* destroyed in milk by holder pasteurization method.
2. *Bacillus typhosus*	
Sternberg, 1887	Veal broth cultures heated in sealed capillary tubes were dead after 10 min. at 56°C.
van Geuns, 1889	Tested in milk in sealed capillary tubes, organisms were dead after 1½ min. at 60°C.; after 5 min. at 56°C.
Hesse, 1900	In milk organisms were destroyed at 60°C. after 15–20 min. Formation of scum prevented.
Bassenge, Meinicke & Friedel, Kolle, Kutscher, 1905	In milk organism was destroyed in 5 min. at 60°C.
v. Drigalski	Organism found viable in milk after heating for 20 min. at 61°–63°C.
Krumwiede & Noble, 1921	When heated in milk, organism does not survive 60°C. for 20 min.
Orskov, 1926	In milk in sealed capillary tubes organisms did not survive 63°C. for 4 min.
Park, 1927	In milk in sealed capillary tubes, all strains tested were killed at 60°C. in 8 min.
3. *Paratyphoid bacilli*	
Krumwiede & Noble, 1921	In milk paratyphoid bacilli did not survive exposure at 60°C. for 20 min.

TABLE 1—*Continued*

INVESTIGATOR	RESULTS
	3. *Paratyphoid bacilli*—Continued
Orskov, 1926	In closed bouillon tubes, 20 strains survived 63°C. for 1–4 min. In open tubes in milk, organisms survived 63°C. for more than 15 min. In closed tubes in milk, dead in 3 min. at 63°C. Dried organisms survived 63°C. for more than 2 hrs. If ring was allowed to form in open tube of milk, organisms were killed at 63°C. in 50 min. but not in 45 min.
	4. *Dysentery bacilli*
Thomson, 1916	Killed bacteria for vaccines by heating at 56°C. for 1 hr.
Runge & O'Brien, 1924	Prepared Shiga toxin by heating aqueous emulsion from agar slant at 58°–60°C. for 10 min.
	5. *Brucella organisms*
Eyre, 1912	Aqueous emulsions of 5 strains of *Br. melitensis* destroyed at 57.5°C. in average of 10 min. exposure.
Zwick & Wedeman, 1913	Agar slants of *Br. abortus* submerged in water bath so that water extended 3 cm. above upper end. Organisms killed in 25–30 min. at 55°C.; in 10–15 min. at 60°C.; in 5–10 min. at 65°C.
Park, 1928	13 strains of *Br. abortus* & *melitensis* (of human, bovine and swine origin) were destroyed in sealed tubes of milk after 5 min. at 145°F., 7½ min. at 142°F. and 10 min. at 140°F.
Carpenter & Boak, 1931	Temperatures of 142° and 145°F. for 20 and 30 min. on virulent strains of *Br. abortus* found satisfactory for pasteurizing milk.
	6. *Bacterium tularense*
McCoy, 1912	Heating liver tissue emulsions in capillary tubes destroyed infectivity in 10 min. at 56°C.
	7. *Hemophilus influenzae*
Onorato, 1902	Destroyed in bouillon and in agar in 2 min. at 62°C.; in 10 min. at 60°C.; in 10 min. at 58°C.
	8. *Vibrio cholerae*
Koch	Boiling temperature destroys organisms instantly. Not viable at 80°C. after 5 min.; at 56° after 30 min.
Kitasato, 1889	Organisms destroyed in 15 min. at 55°C.
	9. *Bacillus pestis*
Kitasato, 1894	Bouillon culture killed in 30 min. at 80°C.; in few minutes at 100°C.
Gladin, 1898	Bouillon cultures in sealed capillary tubes destroyed at once at 70°C.; in 2 min. at 60°C.; in 10 min. at 56°C.; in 60 min. at 50°C.
Kolle, 1912	With concentrated cultures 65°C. for 1 hr. in shaking machine found necessary for certain death.

TABLE 1—*Continued*

INVESTIGATOR	RESULTS
	9. *Bacillus pestis*—Continued
Albrech & Ghou (quoted by Gotschlich), 1920	Heating at 55°–60°C. for 1 hr. gave sterile subculture, but injection of guinea pig caused death of the animal.
Haffkine Institute of Bombay, 1928	Broth cultures are heated for 15 min. at 55°C. for vaccine preparation.
	10. *Staphylococci*
Sternberg, 1887	Heated in broth in sealed capillary tubes for 10 min. *St. aureus* destroyed at 58°C.; *St. citreus* at 62°C.; *St. albus* at 62°C.
Lübbert	Fluid cultures inoculated into water were destroyed after 30 min. at 50°C. Cultures on silk threads killed after 1 hr. at 50°C.
Hesse, 1900	Milk inoculated, then heated for 15–20 min. at 60°C. showed no growth after incubation for 48 hrs.
Samter, 1908	Tests made on 12 strains. Thick suspensions killed at 75°C. after 35–60 min.; thin suspensions, at 75°C. after 15–25 min.
Neisser, 1921	Bouillon suspensions of 24 hr. agar cultures exposed to heat and transferred to fresh bouillon. 5 strains killed in 30 min. at 60°C.; one strain destroyed in 45 min. at 60°C.; in 5 min. at 70°C.
	11. *Gonococci*
Sternberg, 1887	Infected pus rendered sterile by heating at 60°C. for 10 min.
Kiefer, Schäffer, Steinschneider and others	Organism found to die in cultures after few hours at 40°–41°C.
Santos Carlos, 1914	Infected pus heated in sealed pipettes. After 45 min. at 45°C. positive cultures obtained; after 5 min. at 50°C. negative cultures.
	12. *Meningococci*
Bettencourt and Franca, 1904	Bouillon suspension from 24 hr. ascites agar cultures heated in sealed capillary tubes. Dead in 1 min. at 60°C., 70° and 80°C. Viable after 50 min. at 50°C., 3 min. at 55°C.
v. Lingelsheim, 1906	Salt solution suspensions from ascites agar slants were heated. Subcultured to ascites agar. Organisms resisted 50°C. for 1 hr. Destroyed at 80°C. after 2 min.; 70°C. after 5 min.; 60°C. after 10 min.
Elser and Huntoon, 1909	Broth suspensions heated. 4 strains resisted 45°C. for 3 hrs. 2 strains resisted 55°C. for 2 hrs.
	13. *Pneumococci*
Wirth, 1926	16 strains of pneumococci and *Str. mucosus* found to be destroyed at 60°C. in 15 min. Heated in melted dextrose agar.
Bagger, 1926	10 strains tested. Destroyed when 24 hr. broth cultures were heated in sealed ampules at 60°C. for 30 min.

TABLE 1—*Concluded*

INVESTIGATOR	RESULTS
	14. *Streptococci*
	A. Of pathological origin
v. Lingelsheim, 1891	Salt suspension of *Str. hemolyticus* from agar slant viable after 1 hr. at 60°C. 2 hrs. exposure not always effective. *Str. longissimus* destroyed after 10 min. at 60°C.
Simon, 1904	Sediment of broth cultures heated at 63°–64°C. for 1 hr. were rendered sterile.
Ayers & Johnson, 1918	27 strains from septic sore throats failed to survive in milk heated to 60°C. for 30 min.
Dible, 1921	Broth cultures of 9 strains isolated from feces but of salivary origin were killed at 60°C. in 5 min.
Thompson & Meleney, 1924	*Str. hemolyticus* destroyed in 10 min. at 60°C. when heated in dilute salt-gelatine solution.
Beckwith & Rose, 1925	Broth culture of *Str. hemolyticus* 24 hrs. old tested in milk. Old laboratory strain survived 65°C. for 30 min. but not 70°C. After several passages through animals, same strain was destroyed at 60°C. after 25 min.
Wirth, 1926	110 strains from pathological sources exposed to 60°C. in melted dextrose agar did not survive after 15 min.
Bagger, 1926	24 hr. broth cultures of 30 strains heated in sealed ampules at 60°C. for 30 min. were all destroyed.
Park, 1928	100 strains tested in submerged vials of milk were killed after 10 min. at 140°F. and 30 min. at 138°F. Thirty combined cultures heated in milk were destroyed after 10 min. at 140°F.
Tonney, 1928 (reported by Park)	100 strains were tested in submerged vials of milk. After 30 min. exposure, 38 were destroyed at 140°F.; 2 at 138°F.; 60 at 135°F.
	B. Non-pathogenic mesophiles
Orla-Jensen, 1919	*Str. lactis* destroyed in milk at 70°C. after 15 min. *Str. mastiditis* destroyed in milk at 60°C. after 15 min.
Dible, 1921	Broth cultures of streptococci isolated from feces and called enterococci were not destroyed after 15 min. at 60°C.
Seibel, 1925	Cultures of organisms designated as *Str. faecium*, *Str. glycerinaceus* and *Str. mastiditis* survived 63°C. for 30 min. in milk in sealed ampules.
Bagger, 1926	24 hr. broth cultures of 150 strains of enterococci were tested in sealed submerged ampules. 150 resisted 58°C. for 1 hr.; 143, 60°C. for 1 hr.; 100, 62°C. for 1 hr.; 62, 63°C. for 1 hr.; 4, 65°C. for 1 hr.
Robertson, 1927	*Str. lactis* was isolated from milk heated for 30 min. at 145°F. in sealed capillary tubes.
	15. *B. tuberculosis*
North & Park, 1927	Bovine tubercle bacilli grown on glycerine agar and inoculated into milk were destroyed in 6 min. at 145°F., in 10 min. at 142°F. and in 20 min. at 138°F. This report (reference given in the bibliography) contains 2 tables giving complete summary of both the early and recent work on thermal death point of the tubercle bacillus.

that the strains of the same species might have different thermal death points. The culture medium in which the organisms were grown, the culture medium into which the test organisms were subinoculated, the menstruum in which the organisms were suspended, whether physiological saline, distilled water or a colloidal fluid, the thickness of the suspension, the temperature of incubation were all shown to affect materially the results which were obtained (Gotschlich, 1917). The later investigations have added to the confusion. Factors such as H-ion concentrations, the influence of certain anions and cations, the age of the culture when tested and methods of conducting the experiments—whether the organisms are heated in sealed ampules

TABLE 2

Thermal death time of spores of Clostridium botulinum (according to Meyer 1928)

INVESTIGATOR	RESULTS
Van Ermengem, 1897	One strain (Type B) destroyed at 80°C. after 30 minutes.
Thom, Edmondson and Giltner, 1919	One strain (Type A) destroyed at 100°C. in more than 60 min. and less than 120 min. when tested in brain and beef broth.
Burke, 1919	Fifteen strains (Type A) tested in brain broth. Destroyed at 121.3°C. in 10 min.
Weiss, 1921	Sixteen strains (Types A & B) tested in brain broth. Type A survived 100°C. for 20 min. Destroyed in 240 min. Destroyed at 120°C. in 4 min. Type B destroyed at 100°C. in 270 to 300 min.; at 121.3°C. in 3 to 6 min.
Tanner and Dack, 1923	One strain (Type A) destroyed in dried brain in 15 to 60 min. at 140°C.
Dickson and his co-workers, 1922 & 1925	Forty strains of Types A and B tested. Type A destroyed at 100°C. in broth after 240 to 255 min.; in agar, after 170 to 225 min.; in infusion broth with oil seal after 360 to 375 min. Type B destroyed at 100°C. in broth after 125 to 240 min.; in agar after 210 to 225 min.; in brain broth after 270 to 285 min.; in infusion broth with oil seal after 285 to 300 min. At 121.3°C. all strains were destroyed in about 4 min. in broth, agar and brain broth. In infusion broth with oil seal, 22 to 23 min. were required.
Tanner and McCrae, 1923	Four strains (Type A) tested in brain broth under 70 mm. vacuum. Destroyed at 100°C. after 300 min.
Esty and Meyer, 1922	112 strains tested at 105°C. in buffer solutions (pH 7.0). Average death time of Type A was 41.1 min.; of Type B, 23.8 min.
Esty, 1923	112 strains tested in buffer solutions (pH 7.0). Type A destroyed at 100°C. in 320 to 360 min. Type B destroyed in 60 to 70 min. at 105°C.
Bengston, 1924	One strain (Type B) destroyed in 15 min. at 95°C. Six strains (Type C) destroyed in 60 to 90 min. at 100°C. Tests run in beef heart infusion broth.
Starin, 1926	Suspension of 9 to 10 spores of Type B destroyed at 100°C. in 10 min.
Esty, 1927	Four strains of Type B (two billion spores suspended in buffer solution) destroyed at 100°C. in 360 to 370 min. Three strains of Type B suspended in olive liquor destroyed at 115°C. in 20 to 25 min.

or open tubes, or whether the organisms are incubated in the fluid in which they are exposed or transferred to a fresh medium —all have to be considered in a highly critical manner in accepting a given temperature as the thermal death point of the organism in question. In general, however, it seems safe to state that vegetative cells of pathogenic bacteria held at a temperature of 60°C. to 65°C. from ten to fifteen minutes are always destroyed. Spores of aerobic bacteria require a few minutes (one or two—twelve at most) in boiling water or saturated aqueous steam at 100°C. (Ford, 1927). A résumé of some of the most important investigations which have been conducted to determine the temperature required to destroy certain of the more common forms of non-spore forming bacteria is given at the end of this paper (Table 1). The temperatures necessary for the destruction of spores of pathogenic anaerobic bacteria are indicated in tables 2 and 3.

TABLE 3

Thermal death time of spores of anaerobes (compiled from data on 2000 strains tested by Zeissler (1928) and 141 strains tested by Becker (1920))

ORGANISM	RESISTANCE TO STEAM
Cl. welchii (Becker)	8–90 min. in alkaline brain broth.
Cl. novyii (Becker)	8–90 min. in alk. brain broth.
(Zeissler)	About 60 min. in pure culture in brain broth.
Vibrion septique (Becker)	2 to 15 min. in neutral brain broth. In alkaline brain broth 2 to 20 min.
(Zeissler)	30 min. in mixed cultures.
Cl. chauvoei (Becker)	2 to 4 min. in neutral brain broth. 6 to 12 min. in alk. brain broth.
Cl. histolyticus (Zeissler)	60 to 90 min. in brain broth.
Cl. tetani (Becker)	150 to 180 min. in neutral brain broth; 120 min. in alk. brain broth.
(Zeissler)	60 to 180 min.
v. Hibler's bacillus No. VI (Zeissler)	2 to 5 min.
Cl. oedematis maligni (Becker)	Type I: 2 to 4 min. in neutral brain broth. 3 to 6 min. in alk. brain broth. Type II: 180 min. in neutral brain broth. 120 min. in alk. brain broth.
Cl. sporogenes (Becker)	60 to 120 min. in neutral and alk. brain broth.
Cl. bifermentans (Becker)	90 to 150 min. in acid brain broth. 90 to 120 min. in alk. brain broth.
(Zeissler)	40 to 60 min.
Cl. tertium (Zeissler)	2 to 10 min.
Cl. sphenoides	2 to 10 min.
Cl. cochlearium	60 to 180 min.

DEATH RATE OF BACTERIA

Madsen and Nyman (1907) were the first investigators to point out that disinfection proceeds according to the equation used to express the course of a monomolecular reaction. Their data were obtained from the work of Krönig and Paul (1897), who enumerated the anthrax spores surviving after stated intervals of exposure to different types of disinfectants. Chick shortly afterward (1908) expressed the idea that the death of bacteria actually follows the law of a monomolecular reaction, both by the destructive agency of phenol and of hot water (Fig. 2). She

used not only anthrax spores but also certain vegetative organisms. Only one type

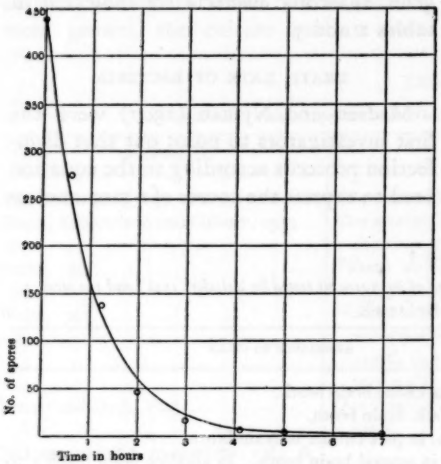

FIG. 2. SURVIVOR CURVE OF ANTHRAX SPORES AFTER DISINFECTION WITH PHENOL AT 20.2°C.
(Chick, 1908)

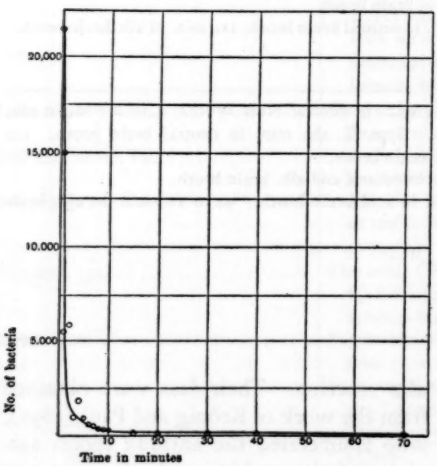

FIG. 3. SURVIVOR CURVE OF 24 HOUR CULTURE OF B. PARATYPHOSUS AFTER DISINFECTION WITH PHENOL AT 20°C.
(Chick, 1908)

of organism tested, *Bacillus paratyphosus*, was found to depart from this simple law (Fig. 3) in that, with this test organism, at first the reaction progressed more quickly than would be expected if the rate were dependent on the concentration of the surviving organisms. After this, however, the accordance with the law was good.

Reichenbach (1911) claims that an explanation of the process proceeding as a monomolecular reaction is based on a false premise—that is, that the number of protoplasmic molecules destroyed would be proportional to the living protoplasmic molecules, which could only be true if the living bacteria were of the same resistance. From experimental data based on suspensions of cells of approximately the same resistance and cells of unequal resistances, he concludes that the rate of death is not according to the law of probability but that an especial explanation is demanded, which explanation rests on the individual resistance of the cells, since the origin of a culture follows a geometric line and the resistance of individual bacteria depends on their age. In answer to criticisms of the analysis of her results, Chick (1910) says that the orderly time process shown from her curves can not be due to variable permanent resistance, because chance characteristics are usually distributed in a different manner; individuals possessing moderate amounts of resistance would be in large numbers, while those possessing large or small amounts of resistance would be in the minority, whereas in her disinfection experiments those possessing the least resistance are invariably in greatest numbers. She believes that a more rational explanation is based on the essential similarity of the individual bacteria; in which case, if the conditions necessary for death are present, then death will occur according to temporary changes in the molecules of the constituent proteins of the bacteria—that is, at a certain moment only a certain proportion are liable to attack.

Loeb and Northrop (1917) are of the opinion that Chick was probably led to the assumption that bacteria are killed at a rate corresponding to that of a monomolecular reaction by the fact that the ascending branch of the mortality curve in her experiments was generally almost a vertical line due to the powerful agencies used for killing the bacteria. The steep ascending branch, therefore, almost escaped detection, and she noticed only the less steep descending branch which could be interpreted as a monomolecular curve for the reason that her experiments lasted only a short while. On the other hand, Cohen (1922) has accepted Chick's main thesis after a series of careful experiments in which he lengthened the initial phase of the mortality curve by submitting certain organisms to sub-lethal doses of toxic salts. Falk and Winslow (1926), however, point out that it is much simpler to assume that lethal reactions which occur in bacterial cells dying in an unfavorable environment proceed in accordance with bimolecular reactions or those of a still higher order rather than in accord with the monomolecular formula.

Reichenbach's contention that the rate of death of bacteria is dependent upon the resistance of the cells has been supported by Peters (1920), Smith (1921), Knaysi (1930) and others. Recently Rahn (1929) has analyzed very carefully the data presented by Reichenbach, Chick and other investigators. He begins his study with the assumption that all bacteria are of the same resistance. He then shows by mathematical formulae that if the death of unicellular organisms is brought about by the inactivation of one or several essential molecules, then the order of death is logarithmic, following that of the mass law, and indicates that in this event the size of the bacteria is responsible for the logarithmic curve. If, on the other hand, the number of reacting molecules becomes larger and larger, his formulae indicate that the order of death should approach that of higher organisms—namely, a period of no death followed by a comparatively short period of rapid death. Since the survival curve of most bacteria, however, is that of a straight line, the author believes that death must be brought about by the inactivation of one molecule, though there may be several molecules of the same type in each cell.

Rahn (1930) then indicates that curves showing a decreasing death rate can be accounted for by assumption of variations in resistance of the cells under test. In Figure 4, the curves of 5.5 and 8 hour cultures of *Bacillus paratyphosus* containing an inhomogenous mixture of cells show deviation from the straight line of logarithmic order; the curves of the 13 and 18.5 hour cultures show a death rate of the straight logarithmic order; while the 24 hour culture shows an increasing death rate. In the two or three instances in the literature of curves showing increasing death rate such as is represented in Figure 5, he calls attention to the fact that staphylococci and spore-bearing bacilli were used as test organisms. He thinks that while death might possibly be due to the destruction of two or three molecules in these cases, it is more probable that these results are caused by our imperfect method of counting living bacteria, since clumps may be counted as one organism in sampling.

Brooks (1919) has expressed perhaps the most valid and pertinent criticism of all the theories which have been elaborated to explain the death rates of bacteria. He asks, "Should we not conclude that the course of processes like disinfection is the result of the simultaneous operation of two factors; the frequency curve of variation in individual resistance, which may be

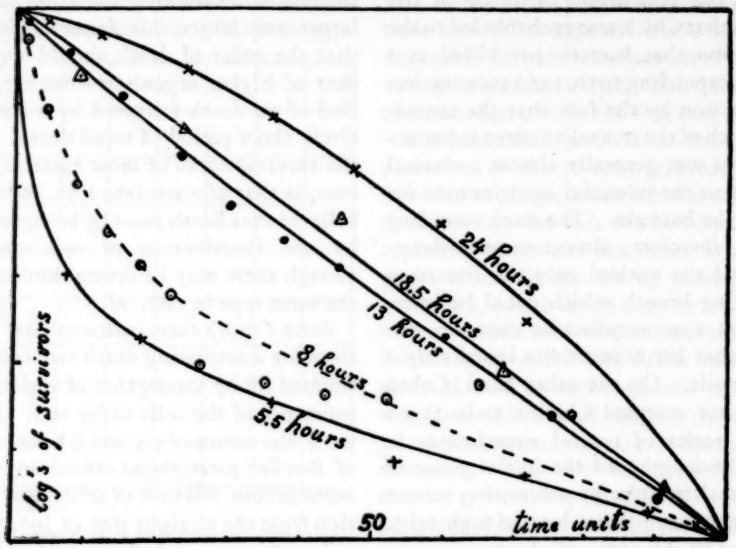

FIG. 4. SURVIVOR CURVES OF B. PARATYPHOSUS CULTURES OF DIFFERENT AGES, DRAWN TO STANDARD SCALE
(Rahn, 1930)

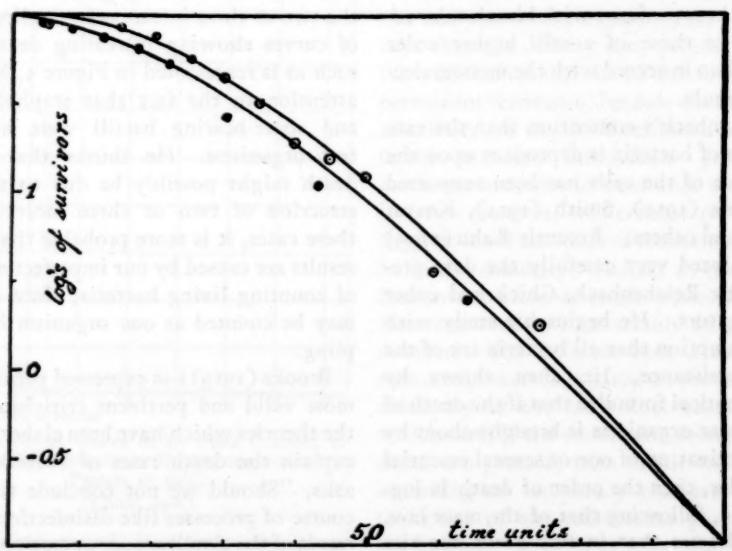

FIG. 5. SURVIVOR CURVES OF STAPH. AUREUS, DRAWN TO STANDARD SCALE
(Rahn, 1930)

different for each group of cells and each toxic agent; and the course of the fundamental reaction, which usually proceeds with a velocity diminished during the experiment at a rate dependent on the particular conditions prevailing?" He says that we must also bear in mind that what we may have supposed to be the fundamental reaction may be the end result of a complex series of interrelated reactions.

From the evidence presented, then, it would seem that the thermal death curve of many bacteria, including certain thermophiles (Bigelow and Esty, 1920), appears to be of a logarithmic nature. Whether this represents a coincidental phenomenon or whether it is of fundamental significance remains in doubt at the moment. The exceptions to the rule are of such great importance that the continued study of a large number of species of bacteria of biologically and chemically different natures under a variety of conditions, such as has been initiated by Winslow and his co-workers, seems warranted.

HEAT RESISTANCE OF BACTERIA

The whole subject of heat resistance in organisms has been admirably reviewed in detail by Robertson (1927). Thermal resistance of spores specifically has been reviewed by Magoon (1926). In general, it is recognized that young cells are more easily destroyed than old cells, and that spores are more resistant than vegetative forms. Dry spores are less readily killed than moist spores. The proof for these statements is based on several facts, the most important of which is that the thermal resistance of a cell is dependent to a great extent on its water content. The cell wall, acting as a limiting membrane, therefore, plays an important rôle. Differences between the water content of vegetative cells and spores have been amply demonstrated, but the precise influence of the cell membrane on heat resistance has been more difficult to establish. Cramer (1894) suggested that the resistance of spores to dry heat depends on the retention of hygroscopic moisture by the cell wall, while the resistance to moist heat is due to the effect which the spore wall exerts on the penetrating moisture as it passes through to the albumin. Cramer's theory of difference in the permeability of cell membranes has been substantiated by subsequent work on the relative penetration of dyes into bacteria. Many investigators have demonstrated that dead spores stain more readily than live spores because their membranes have lost the property of retarding the passage of the moisture in which the dye is suspended. Burke (1923) has likewise indicated that the degree of permeability of the vegetative cell wall to water must have an important influence on heat resistance.

It has been suggested from time to time that another important factor which aids organisms in resisting destruction by heat is the existence of a protective coating about the cell. This may be in the nature of an inherent component of the bacteria such as a mucoid secretion or a capsule or else it may be an artificial covering such as is known to exist when bacteria are heated in milk or in sublethal sodium chloride (Viljoen, 1926) and sucrose solution (Robertson, 1927 a). Von Esmarsch (1889) expressed the idea that spores surround themselves with a firm protective coating which partially prevents the penetration of heat. Lewith (1890) rejected this idea and was unable to conceive of a coating of sufficient thickness or the proper composition to protect such small objects as bacteria from heat penetration. Subsequent investigations, however, have indicated that protective coatings do exist, though their mode of insulation is unlike that suggested by Von Esmarsch. Such

coverings probably prevent the coagulation of the albumin when heat is applied either by withdrawing the moisture already in the cell or by retarding its passage inward through the cell wall. Hückel (1926) has made the interesting observation that in old cultures of *Bacillus coli*, *Bacillus pyocyaneus* and staphylococci a protective substance is given off into the suspension fluid by the cells. This material, which is non-specific, even when separated from the bacteria by filtration imparts greater heat resistance to less resistant organisms.

Aside from the theoretical interest which exists in a study of thermoduric and thermophilic bacteria, certain practical considerations have arisen in the pasteurization of milk and in the canning industry.

PASTEURIZATION OF MILK

The literature on the subject of the proper pasteurization temperature, the relative merits and defects of the holder and the flash methods of pasteurization, the bacteria which survive pasteurization and the factors which prevent the death of organisms in milk at pasteurization temperatures is vast and confusing. Ørskov (1926), North and Park (1927) and Reichel (1929) have recently made adequate reviews of the situation.

Pasteurization of milk was introduced as a safeguard against recognized milk-borne diseases, chief of which in the early days were tuberculosis and infections due to pathogenic organisms of intestinal origin. Since then certain respiratory pathogens are also known to be milk-borne frequently. In any discussion it should be emphasized in the first place, that the question of survival or death of microorganisms in laboratory pasteurized milk is not identical with that of survival in a pasteurizing plant where the bulk of the milk is large and the method of handling different. The question of strains of organisms also arises. In laboratory procedures many strains may be employed and found to be destroyed at pasteurizing temperatures, yet we are always faced with both the possibility and the probability of the existence of heat-resistant strains of the same species. The margin of safety necessary to meet such a contingency then becomes of primary importance and significance. This is amply illustrated by research which has been done on *Bacillus coli* and *Bacillus tuberculosis*.

In the early days of milk control, the chief aim of pasteurization was the destruction of the resistant tubercle bacillus, since it was admitted that temperatures sufficient to render this organism innocuous would kill other pathogenic bacteria. The first systematic investigation concerning the survival or death of this organism in milk was conducted by Bang (1885), who observed that a temperature of 70°C. and 72°C. for five minutes was sufficient for destruction, but not 65°C. for the same length of time. The high thermal resistance of the tubercle bacillus in milk as indicated in this and other experiments which appeared in the earlier literature was no doubt due to the methods used, since at that time little was known concerning the extraordinary protective qualities which might exist in milk due to the formation of a scum, dried particles on the sides of the container, the occurrence of flakes in the milk, etc. In 1888 Yersin heated tuberculous milk in sealed tubes and found that 60°C. for ten minutes partly destroyed the organisms, while 70°C. for ten minutes brought about complete destruction. Galtier (1888) performed a similar experiment and was not certain that the organisms were killed at 60°C. in twenty minutes or 70°C. in ten minutes. Bitter (1890) showed that flash pasteurization from 75°C. to 80°C. was unsatis-

factory practically, and the latter introduced the idea of holder pasteurization methods with constant stirring at 68°C. for thirty minutes. In a series of careful and precise experiments Forster (1892) and his pupil de Man (1893) found that the tubercle bacillus was killed at 55°C. in three to four hours, at 60° in forty-five to sixty minutes, at 65° in fifteen minutes, at 70° in ten minutes, at 80° in five minutes, at 90° in two minutes, and at 95° in one minute. Bonhof (1892) showed that 60°C. for twenty minutes was sufficient. This work was confirmed in this country by the admirable experiments of Smith (1899), who was the first to point out the necessity of prevention of the formation of a scum which protects the test organisms from heat penetration.

The thermal death points of the tubercle bacillus as indicated by Forster's and de Man's work led to the introduction of holder pasteurization methods in Holland. These methods were severely criticized by de Jong (1909), Basenau (1910) and van der Sluis (1909) who revealed the presence of living organisms in milk heated in a closed flask for thirty minutes at 65°C. and centrifugalized. Forster (1910) and Aoki (1910) repeated the earlier work of Forster and ascribed the results of de Jong and others to poor technic in using an open water bath and flasks not entirely submerged. These workers appear to have been entirely unaware of the extensive work which had been done by the Scandinavian milk bacteriologists Barthel and Stenström (1901) in coöperation with Bang on feeding experiments with tuberculous milk. These investigators showed the importance and significance of flocks in milk. Heating milk free from flocks to 80°C. for one minute was found sufficient to destroy the tubercle bacillus, whereas a much longer time was required for milk containing flocks. These experiments were repeated in 1912 and identical results obtained.

In this country the subject initiated by the work of Theobald Smith has been subsequently investigated by Rosenau (1908), Traum and Hart (1916) and Park and his co-workers from the standpoint of holder pasteurization methods (North and Park, 1927). These results have indicated that properly pasteurized milk is incapable of transmitting tuberculosis. Park and his followers are of the opinion that a temperature of not less than 142°F. for thirty minutes is sufficient. Bang (1901) and others have shown that exposure of tuberculous milk to a temperature of 140°F. for five minutes is sufficient to prevent the development of tuberculosis when consumed by mouth rather than injected into the tissues of the body. Park's ideas are adequately substantiated by epidemiological studies done in New York and other cities. On the other hand, there are still many workers both in this country and abroad who believe that the temperatures commonly used for pasteurization in both the flash and holder methods are not adequate for safety (Richter and Seelemann, 1926; Meanwell, 1927).

In general, the evidence presented seems meager and the amount of material insufficient when compared with the comprehensive investigations of Park and his co-workers (Park, 1927).

Other pathogenic bacteria of intestinal and respiratory origin are rapidly destroyed in milk elevated to a temperature sufficient to destroy *Bacillus coli* (Hesse, 1900; Krumwiede and Noble, 1921; Park, 1927). However, the destruction of *Bacillus coli* in milk has also led to much discussion. In laboratory experiments, this organism has been found to be killed at a pasteurization temperature of 142°F. for thirty minutes or less by practically all careful investigators, whether the milk is

naturally or artificially infected with the organism. On the other hand, milk obtained from adequately controlled pasteurizing plants where the total bacterial count is low has repeatedly been found to be infected with *Bacillus coli* if the proper medium and technic for isolation have been employed. Indeed, de Jong and de Graef (1914), Ayers and Johnson (1914) and Shippen (1915) have isolated strains of bacteria from pasteurized milk, which they identified as *Bacillus coli*, whose thermal death points were from 10 to 15 degrees higher than the temperatures used in pasteurization. In no instance has such a highly resistant organism been isolated from raw milk. The question which naturally arises from such observations is the likelihood of development of heat resistance in strains or the survival of a few highly resistant cells during the pasteurization process. With this in mind, Swenarton (1927) and others have proposed the use of the *Bacillus coli* index as a control on proper pasteurization. Such a test not only accepts the existence of strains of the organism which resist 142°F. for thirty minutes, but also admits that a few organisms may always survive without endangering the health of the consumer. There are, on the other hand, many workers who believe that there is insufficient evidence of the existence of heat resistant *Bacillus coli* and that the finding of even small numbers of this organism in milk is indicative of either faulty pasteurization at 142°F. or that a minimum standard of 142°F. is too low for safety. Ayers and Johnson (1924), for instance, call attention to the fact that there may be a wide range between the "majority" and "absolute" death points of organisms and that this should be given careful consideration in determining the pasteurization temperature capable of destroying pathogens. This is undoubtedly a very important point since milk acts as an ideal culture medium for many pathogenic bacteria. A few surviving cells might well multiply and become present in sufficient numbers in a bottle of milk to give rise to an infectious disease when consumed.

Milk itself possesses certain properties which act as a protection for bacteria against death by pasteurization. According to Seibel (1925) the protective elements found present in milk which prevent complete sterilization are: the colloidal nature of the milk itself which forms a covering for the microorganisms; the clumping and mucoid properties of the bacteria; spore formation; and the existence of an acid-free lab ferment. The inherent qualities of heat-resistant organisms must also be taken into consideration.

The microorganisms which ordinarily survive pasteurization are lactic acid bacteria, sarcina, bacterial spores and thermophiles (Orla-Jensen, 1919; Ayers and Johnson, 1918; Robertson, Yale and Breed, 1926; Robertson, 1927 b; Rogers and Frazier, 1930). Ayers and Johnson (1924) isolated a thermophilic organism from pasteurized milk and suggested that thermophiles might perhaps cause trouble in pasteurizing plants. The frequency of their occurrence in pasteurized milk in this country was simultaneously pointed out in 1927 by Eckford and Robertson (1927b). Since then Breed and his co-workers (Breed, 1928; Prickett, 1928; Hucker, 1928–29; Prickett and Breed, 1929; Yale, 1929) have devoted a great deal of attention to the subject of thermoduric and thermophilic bacteria in their relation to the pasteurization of milk. The presence of true heat tolerant bacteria in pasteurized milk in large numbers is thought to be the result of faulty or undesirable rather than unsanitary plant practices. Though the original source of the organism is connected with the inoculation of the milk

with dirt, dust, fecal material, etc., there is no evidence that the presence of these bacteria in milk even in large numbers is dangerous to human health and well being. They do produce undesirable qualities in the milk, however, since they may cause a pronounced acidity or alkalinity to develop and are frequently proteolytic in their action.

HEAT RESISTANCE IN THE CANNING OF FOOD PRODUCTS

The problems involved in the canning of food products differ from those of the production of pasteurized milk in that sterilization is the chief aim of the producer, for it is only after complete sterilization that a food product may be considered safe from spoilage. Up until 1918 only sporadic investigations were made on the bacteria found in canned goods. The literature on this subject has been reviewed in detail by Weinzirl (1918), who examined 92 cans of vegetables, 49 of which he considered fit for eating. Of these 33 were free from organisms. Cheyney (1919) examined 725 cans of merchantable foods. Eight per cent were found to contain living organisms. Ten to twenty per cent of the fish and meat products investigated contained viable organisms; three per cent of the fruits and eight per cent of the vegetables.

It is clear from these two investigations alone that each product furnishes a problem within itself. The time and method used for processing is primarily different for each food (Weiss, 1921; Esty and Williams, 1924; Ball, 1928). The nature and consistency of the food, the qualities of the liquor or syrup in which the processing is carried out and the size and shape of the cans or jars are important in that they affect the penetration of heat. Whether the food is solidly or loosely packed so that heating may occur both by conduction and convection is of significance in determining the time required to bring about sterilization. The H-ion concentration of the raw material also has a direct bearing on the temperature and time required to destroy the bacteria present, since it is recognized that the presence of acids lowers the resistance and shortens the time interval required to accomplish the destruction of both spores and vegetative forms. The type of the native bacterial flora serves as a criterion for the temperature adequate for sterilization, while the initial concentration of organisms present, which is dependent on the handling of the food, determines the length of time the heating should continue, since the chances of securing a larger number of resistant organisms increase with the increase in the total number of cells (Esty, 1923).

The various phases of the problems have been well recognized by the commercial canning factories and have been the subjects of many investigations initiated by them, particularly with reference to the organism which causes botulinus poisoning. It is unfortunate that this information has not been as yet so well disseminated as to be used to advantage by the housewife who does home canning. In many households the loss of food products through spoilage is very high each year. Aside from the loss of time, energy and material which occurs, there is also a serious situation created by the potential danger of botulinus poisoning due to improper processing, since deaths due to this poisoning each year are almost invariably traced to home canned foods.

Burke (1919) called attention to the fact that fractional sterilization, the method of canning by boiling the fruit or vegetables in open kettles and sealing in clean jars, and the cold pack method can not be relied upon to kill the spores of

Bacillus botulinus. Pressure canning was considered the only safe method for household use and a comparatively long sterilization period was shown to be necessary.

The organisms which have been found responsible for the spoilage of canned foods are in general mesophiles, aerobic and anaerobic spore-formers and thermophilic bacteria—all usually derived from the soil. The thermophiles are ordinarily causative agents in the condition of food spoilage known as "flat sours," where there is a production of acid without the formation of gas. The mesophilic aerobic and anaerobic spore-formers are commonly implicated in putrefactive processes and in the production of gas (Esty and Stevenson, 1925; Cameron and Esty, 1926; Fellers, 1926; Townsend, 1929).

The only pathogenic organism known to cause the spoilage of canned food is an anaerobe, *Bacillus botulinus*, since other pathogens are readily destroyed by the high temperatures used in canning. This organism, which produces spores highly resistant to heat, is harmless when the organisms are ingested. Its danger in food products lies in the production of an extracellular toxin which is extremely potent and for which an anti-toxin possesses only limited therapeutic value. The voluminous literature on the subject which has grown up since 1918 has been admirably reviewed in German by Meyer (1928) who with his co-workers has made invaluable contributions in this country. All the available fundamental knowledge on the heat resistance of bacteria has been applied experimentally to *Bacillus botulinus*, and thus the commercial canners have been able to acquire very accurate information on the temperatures and length of time necessary to safeguard food products from botulinus poisoning. The heat resistance of other anaerobes has not been found to be greater than that of *Bacillus botulinus* (Esty and Meyer, 1922; Esty, 1923), hence temperatures which kill this pathogen also protect the food from spoilage due to other anaerobic mesophiles. The resistance of the spores of many of the anaerobes is given in Tables 2 and 3.

An interesting and fruitful field for further research in the bacteriology of foods has been indicated by the recent investigations of Shaw (1928), Cameron and Williams (1928–1929) and Prickett (1931), who have pointed out the significant rôle of obligate and facultative thermophiles in many instances of food spoilage.

THE EFFECT OF LOW TEMPERATURES ON BACTERIA

The existence of psychrophilic bacteria has long been recognized, but the more recent studies have been limited in character to the accidental finding of certain species in spoiled foodstuffs kept at temperatures below freezing. Their specific rôle in nature, if any, has never been systematically investigated.

On the other hand, organized studies on the effect of low temperatures on the growth and survival of mesophilic bacteria both in nature and in laboratory cultures have been made from time to time, from which very definite general conclusions have been reached. Spores, because of their low water content, remain almost unaffected by low or freezing temperatures. Vegetative forms of different species, however, are known to vary in their reactions to an unfavorable environment. Some bacterial cultures die off rapidly when placed at temperatures 5 to 10 degrees below their optimum for growth,—for example the gonococcus and the meningococcus. Other organisms are known to multiply regularly though slowly. Presumably the weaker cells are destroyed, but the more resistant forms are actually able

TABLE 4

Viability of certain mesophiles at low temperatures

INVESTIGATOR	RESULTS
	1. *Bacillus coli*
Buchner, 1885	Bacilli survived −20° to −24°C.
Park	Aqueous suspension in liquid air grew after 3, 20, 60 and 130 min. Greatly reduced in numbers.
	2. *Bacillus typhosus*
Prudden, 1887	Few organisms remained alive in ice up to 22 weeks.
de Jong, 1922	Freezing of meat and fish did not destroy organism at −120°, −90° and −153°C.
Prucha and Brannon, 1926	Organism recovered from ice cream kept at temperature of −4°F. for 2 yrs., 4 mos.
Park, Williams and Krumwiede, 1929	21 strains tested. Water inoculated, frozen and then kept at temperature of 20°–28°F. Six cultures sterile after 5 wks.; 4 alive after 16 wks.; all sterile after 22 wks.
	3. *Dysentery bacilli*
Pfuhl, 1902	Recovered bacilli from infected feces and earth mixtures after 101 days at a temperature of 1.5°C. to 15°C.
Schmidt (quoted by Flexner, 1907)	Failed to isolate organism from mixtures of earth and water or milk exposed to full degree of cold of winter. Growth of Shiga bacillus inhibited at 5° to 6°C.
	4. *Hemophilus influenzae*
Onorato, 1902	Destroyed in blood broth after 2½ hrs. at −15°C.; after 1½ hrs. at −20°C.
	5. *Brucella bronchisepticus*
Torrey & Rahe	Broth cultures exposed to cold of winter (mean temperature of 0°C. or below, with frequent periods below −12°C. and once at −19.5°C.). Alive after 24 days. Dead after 43 days.
	6. *Vibrio cholerae*
Uffelmann, 1893	Organism alive and infectious at 0°C. for 12 days; at −25°C. for 5 days.
McFadyean, 1900	Organism not destroyed by −183°C. in 7 days.
	7. *Bacillus pestis*
Gladin, 1898	Cultures held at −22°C. with daily thawing and freezing for 40 days remained viable.
Kasanski, 1899	Agar cultures held at −31°C. for 5½ months without loss of viability. Fully frozen for 4 mos. One culture thawed 8 times and, again frozen, showed weakening in virulence.

TABLE 4—*Concluded*

INVESTIGATOR	RESULTS
8. *Staphylococci*	
Neisser, 1919	Repeated freezing at −30°C. and thawing failed to cause death.
Park, Williams and Krumwiede, 1929	30% of organisms remained viable after being subjected to freezing in liquid air for 30 min.
9. *Gonococci*	
Lumière and Chevrotieri, 1914	Old cultures viable after being held for 10 days at −20°C.; for 24 hrs. in liquid oxygen (−195°C.).
Lorentz, 1924	3 strains submitted to average temperature of 4°, 6° and 7°C. survived 9–15 days. Organisms recovered from infected pus placed at temperature of 5°–6°C.
10. *Meningococci*	
Elser and Huntoon, 1909	May remain alive for years if dried rapidly under freezing temperatures and kept frozen.
11. *Bacillus diphtheriae*	
McFadyean and Rowland (quoted by Ford, 1927)	Survived temperature of liquid air (−190°C.) for 10 days.
12. *B. tuberculosis*	
Galtier, 1888	Survived 3°–8°C.
Calmette, 1928	Viability and virulence not destroyed at temperature of liquid air.

to increase to astonishingly large numbers. The recent investigations on this phase of the subject have been augmented by the introduction of electric refrigeration where foodstuffs are supposedly kept free from bacterial growth by low temperatures. The work of Damon and Leiter (1927) on the storage of certain foods previously inoculated with the *Salmonella* group of organisms in ordinary refrigerators has indicated that such bacteria multiply progressively in non-acid foods even under an unfavorable environment. Prescott and Bates (1931) found that certain types of spoilage organisms adapt themselves to temperatures assumed to be inhibitive to decomposition processes under conditions of domestic refrigeration. In electric refrigeration cabinets where the temperatures are lower and more accurately maintained, Prescott, Bates and Needle (1931) have observed that growth in various foodstuffs occurred, especially if the refrigeration was discontinuous.

The preservation of bacterial cultures by storing at low temperatures has been practiced since the beginning of bacteriology, and it has been assumed that such preservation is due to cessation of metabolism on the part of the organisms. This idea is further substantiated by the observation that cultures stored at low temperatures under vacuum remain viable and retain their virulence for longer periods than those exposed to the air.

It is also recognized that freezing does not destroy bacteria, especially when the change is brought about rapidly. On the

other hand, alternate freezing and thawing disrupts the relation of the water and the protein molecules to such an extent that death is more apt to occur than when freezing temperatures are maintained over a long period of time.

The ability of certain pathogenic organisms in nature and under artificial conditions to survive temperatures below freezing has been demonstrated repeatedly. A summary of the more important observations is given in Table 4. As can be seen, the majority of the investigations have been made on those bacteria of intestinal origin which are apt to survive in ice and become infectious through the alimentary tract. Infections thus acquired are not numerous, though they are entirely possible, especially when the infection of the ice is of recent occurrence prior to use.

The scope of a paper of this type has been necessarily limited to a brief review of our general notions of the influence of temperatures on the life processes and death of bacteria. The technical points of many phases of the subject have been dealt with comprehensively by other writers from time to time, and the bibliography, though by no means complete, is intended to give the reader easy access to any portion of the literature to which his interest may be particularly directed. The following books were consulted freely: Ford, *Textbook of Bacteriology*; Loehnis & Fred, *Textbook of Agricultural Bacteriology*; Buchanan & Fulmer, *Physiology and Biochemistry of Bacteria*; Vols. I and II; and Gotschlich, *Physikalische Desinfection in the Handbuch der pathogen Mikroorganismen of Kolle & Wassermann III, 2 Aufl.*

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THE POPULATION OF A NEST OF THE HORNET VESPA MACULATA

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THE somewhat exclusive nature and temperamental behavior of the social Vespidae in their intimate contacts with human society, combined with other difficulties, make a continuous and complete study of a wasp or hornet nest, throughout the various phases of its existence during a single season, practically impossible. It is only by obtaining for study many samples of nests at different stages of their development, that the whole life-history of a colony may ultimately be reconstructed. Based on this procedure, much information has already been accumulated. It is known that the social Vespidae comprise two general groups (Comstock *et al.* :30, pp. 367–368): the social *Polistes*, whose nests are a single comb, and the social hornets and yellow jackets, whose nests are respectively arboreal and underground. The origin of a colony and much about its development have been learned. It is known that in the spring a single queen begins a new nest, building a few cells, in each of which an egg is laid. These eggs develop into workers, who take over the task of enlarging the nest. The queen continues laying eggs, using the new cells as they are constructed. Towards the end of the summer larger cells are built, usually in the first combs. From eggs laid in these cells the new queens are hatched. Finally the mother queen lays unfertilized eggs, from which the drones develop. With the first cold weather, the drones fertilize the new queens and die. The old queen dies, the workers cease feeding the young larvae, even destroying them, the colony becomes disorganized and only the young, newly fertilized queens remain. These hibernate until the following spring, when the survivors fly forth, each to establish a new colony. (Réaumur, 1742; Ormerod, '68; Fabre, :19; Wheeler, :28).

Information on American social wasps is somewhat sparse. The beautiful studies of the Peckhams ('98) on the solitary wasps and those of the Raus on the social species are outstanding in their respective fields. British contributions to the biology of the social Vespidae are numerous, and it may be noted that many have been made by curates. The American clergy might well emulate so commendable a precedent. The main outlines of the picture have been drawn, but with a consideration of more specific points, such as those relating to size of nests, population of nests, and the relationship between the two, gaps appear and apparent discrepancies are found. A survey of the literature will show why answers to questions on these points must still be tentative and speculative in part, and why further investigation is essential.

THE SIZE OF THE NESTS IN THE VESPIDAE

In his remarkable work published in 1742, Réaumur writes (p. 169): "Tous les guêpiers ne sont pas composés d'un nombre égal de gâteaux; j'en ai trouvé jusqu'à quinze à quelquesuns, et onze seulement à d'autres." He estimates that a nest of 15 combs contains a total of 16,005 cells.

Crawshay (:05), speaking of a nest of *Vespa vulgaris* which he found in the earth, says (p. 10): "The cells did not appear to be more numerous than those of other completed nests, one of which close by, I counted, numbering 7,000 cells." Oudemans (:01, p. 98) also describes a nest of *Vespa vulgaris*, found on a roof. Its height was 21.5 cm., its length 49 cm. and its width 29.5 cm. Its volume was computed to be 36,108 cc. This volume was computed by multiplying the height by two different diameters, a method which he recognizes as inaccurate and valuable only for comparison with other volumes computed by the same method. He quotes Janet ('95) as follows: "D'après Rouget ['73], les dimensions maxima des nids de *V. vulgaris*, sont celles d'une sphère de 30 cm. de diamètre avec douze gâteaux et 20,000 alvéoles. . . . Le plus grand des nids de *V. vulgaris* capturé par Kristof ['79] avait 22 cm. de diamètre, 30 cm. de hauteur et comprenait douze gâteaux." Oudemans also (p. 99) takes the statement of André ['81] that nests "ne dépassent guère 20 à 25 cm. de diamètre, rarement 30," and computes the volume at 27,000 cc. He finds that the volume of Kristof's nest, mentioned by Janet, is 14,520 cc. Comparatively speaking, the nest of his study, he concludes (p. 100) "wirklich kolossal war."

Nests of *Vespa vulgaris* were also studied by Ritchie (:15, p. 319) throughout a season. In July, a nest was found 8 inches in diameter. It had 6 combs which contained no drone or queen cells. The total number of cells was estimated to be 4,217. A September nest found in Scotland was (p. 328) 9 inches in diameter, and composed of 6 combs and 5,312 cells. An October nest had (p. 329) 9 combs and 11,560 cells.

Pack-Beresford (:04, p. 242) records an underground nest of *Vespa rufa-austriaca* which had two combs, the upper of which contained over 200 small cells and the lower 100 queen cells.

Ormerod ('68, p. 21) gives the dimensions of a nest "larger than any I have seen elsewhere." The transverse diameter was 15 inches (38.1 cm.) and the height 19 inches (48.28 cm.).

Fabre (:19, p. 258) says that in wasp-nests the "total number of cells amounts to thousands." He cites one sample which, by actual count, contained 11,900 cells.

Wyman ('60, p. 414) reports on a nest, thought to be that of *Vespa germanica*, taken on October 12 at Cambridge, Massachusetts, which contained four combs.

Marlatt ('90, p. 81), describing subterranean nests of *Vespa germanica* found at Takoma, Maryland, says that they are composed of from four to eight tiers of combs. He states that "in the larger nests opened these combs had a maximum diameter of twelve inches, and there were as many as eight tiers."

For the present study, records of nests of the American hornet, *Vespa maculata*, are of especial interest. McCook (:02, p. 434) describes such a nest. He says: "The specimen . . . . was eighteen inches long and a foot in diameter at the thickest part." A second nest was opened and found (p. 437) to contain six combs of cells.

Davis (:19, p. 119), found a November nest of *Vespa maculata*. He says of it that "it was two and one-half feet in length," and states that "no other species of native wasp would have constructed so large a domicile."

Parker (:28, p. 14) found a November nest of *Vespula maculata* and "from it removed the combs, four in number, placed one above the other." He refers to this as a large nest.

Rau (:29, p. 669) took a nest of *Vespa maculata* in September. It contained four combs and 1,139 cells.

Baerg (:21) contributes notes on a nest of *Vespa diabolica* found at Fayetteville, Arkansas. In reference to this species he says (p. 509): ". . . . these build a nest not unlike that of various species of Polistes; but locate it in the ground instead of in some tree or shrub." A nest taken late in September consisted of six combs. The base was ten and one-half inches in diameter. Another nest (p. 510) taken late in August also contained six combs.

Manee (:15, p. 266) found a nest of *Vespa cuneata* in Southern Pines, North Carolina, late in October. The nest contained five combs.

The single comb nests of *Polistes* wasps, as would be expected, are much smaller than those of *Vespa*. Isely (:22, p. 341) describes a nest found at Bentonville, Arkansas, which on July 10, contained 32 cells. By the end of the season there were 82 cells, which he considers a fairly representative number.

Cornel Schmitt (:19, p. 112) states that the largest *Polistes* nest in his experience contained 163 cells. He compares this with Janet's and Kristof's "200 als Höchstzahl," and with an old Italian nest which had been brought to him, which contained 220 cells.

Rau and Rau (:18, Ch. VII) record two nests of *Polistes annularis* found by Pierce (:09, p. 17) in Texas, one of which contained 1,575 cells, the other 1,212 cells.

Savin (:25, p. 81) writes: "A large nest of *Polistes annularis* contained about 1,500 cells: one of *Vespa germanica* had about 14,000 and the huge nests of *Vespa maculata* have many more." He also mentions (p. 85) a *Polistes* nest taken in August, which contained 135 cells.

Davis (:19, p. 122) refers to a *Polistes* nest found in May which contained twelve cells. He mentions other May nests ranging in size from three to fourteen cells.

One other nest, reported by Rau (:31, p. 85), may be cited to show that ". . . . the wasps can adapt themselves to new environmental conditions in many ways." This nest, of *Vespa germanica*, was built on the floor of a tool closet and contained "not several combs, one beneath another, but one large comb." This comb was about three times the normal width.

Reviewing the above data, wide variation in size is seen to exist, not only between nests of different species of Vespidae, but even within the same species. The number of combs ranges from two (Pack-Beresford) to 15 (Réaumur); the number of cells from the 32 of the *Polistes* wasp (Isely) to Fabre's actual count of 11,900 and Réaumur's estimated total of 16,005 and Rouget's 20,000. It is not surprising to find differences for different species, and it is probable that as more cases are accumulated, the variation within a species will approach a normal curve.

THE POPULATIONS OF WASP-NESTS

Information in the literature on the number of wasps inhabiting a nest reveals widely diverse counts, and brings to attention certain problems. Population statistics have been based in some cases on actual counts of insects found in a nest, and in others on calculation of what the probable count would be. Réaumur (1742, p. 170), basing his estimation on the assumption that "il n'y a peut-être pas de cellule qui, l'une portant autre, ne serve à élever trois jeunes guêpes," says: "Ainsi un guêpier produiroit par an plus de trente mille guêpes," the nest in question having 10,000 cells.

Crawshay (:05, p. 8) actually counted the insects in a nest of *Vespa vulgaris*, and found 4,957 adult wasps. To this

number he adds 250, the number he estimates were not recovered, thus bringing the total number of adults to 5,207. He observes that either figure (4,957 or 5,207) is "considerably in excess of F. Smith's estimate of 2,590–2,690 for a large community, while it in no way approaches to Réaumur's 30,000 for the same." He continues: "I am inclined to think that F. Smith's estimate will hold good for any ordinary English summer of normal conditions of weather, and that only unusually favorable conditions of temperature etc. would produce any appreciably larger number in a community." Crawshay's nest was destroyed on September 20th, and a differential count of the inhabitants gave (p. 8) the results shown in Table 1.

TABLE 1

Crawshay's data on nest of Vespa vulgaris

	LARVAE	PUPAE		ADULT HORNETS			GRAND TOTAL
		Drones Workers	Queens	Drones	Queens	Workers	
No.	1,201	2,280	2,594	1,107	551	3,299	11,032
Totals	1,201	4,874		4,957			11,032

The addition of 314 eggs and 24 beetle parasites which were found brings the total count to 11,370.

Ritchie's population counts for the three nests of *Vespa vulgaris* described above are: for the July nest—417 workers and 1 queen; for the September nest—694 workers, 503 queens and drones, 652 larvae and 680 pupae, a total of 2,529 individuals, of which 1,197 were adults. The population of the October nest is not given, but the statement is made (p. 329):"—Mr. Evans estimated that during the course of the season this huge nest would have produced not less than 25,000 wasps."

Fabre (:19), discussing the point that probably only a few queens survive the winter, says (p. 268): "This was to be expected. . . . One female is enough to found a city of 30,000 inhabitants. If all were to prosper, what a scourge!"

Wheeler (:28, p. 74) places the probable population of a nest in a season at from 3,000 to 5,000.

Wright, Lee and Pearson (:06, p. 407), speaking of a nest of *Vespa vulgaris*, say: "—it contained 129 perfect queens, upwards of 150 perfect drones, and many hundred workers." And Thomson, Bell, and Pearson (:11, p. 2, footnote): "The number of queens in the autumn nests may run from 30 to 100 or 200, and even in special cases, as those of two nests of *Vespa germanica* sent to us last year, to 500 and over 600 respectively."

Wyman ('60, p. 414) gives the census count of his October nest attributed to *Vespa germanica*, described above. He found 24 queens, 236 males, and 388 workers, a total of 648 insects.

Baerg (:21, p. 509) took the census of his September nest of *Vespa diabolica*. He says: "The inhabitants of the nest numbered 2,090. Of these there are 294 queens, 771 males and 1,025 workers. In the lower two stories there are still a large number of cells containing immature queens." In his August nest (p. 510)

"the population numbered 146, consisting of 142 workers, three males, and one queen."

Duncan (:24, p. 41) collected a nest of *Dolichovespula diabolica* at Requa, California in June. It contained 39 workers. In a July nest collected at Dodson, Oregon, he found 3 queens, 4 males, and 107 workers.

Rau (:29) watched the construction of a nest of *Vespa maculata* from May 28. On June 13 the queen was the sole inhabitant. On June 18 (p. 662) three workers were present and "the number of cells within had increased greatly." A census was taken (p. 668) of the September nest referred to above, after the hornets had been etherized. Thirty-four queens were found and 235 workers. The young in the capped cells were uninjured by the etherizing. These were hatched in a cage to the number of 85 queens, 52 males and 88 workers. The total adult population at the end of the season was therefore 494. Rau says (p. 668): "One is justified in assuming that the number 494 is approximately correct for this nest at the end of the season."

Manee (:15, p. 265) found an October nest of *Vespa cuneata* which contained 35 queens, 330 males and 174 workers.

Pechlauer (:04, p. 77) counted the wasps in a September nest of *Vespa germanica* and found 32 queens, 151 drones and 1,165 workers, a total of 1,348 adult insects.

Here again, diversity of results over a wide range is evident. Statistics vary, for actual counts, from 114 insects to 11,370; estimates range from 2,500 to 30,000 or even more. Two problems are suggested, one being the question of relationship between the number of cells in a nest and the total population. The other raises the point of whether there is a distinction between the maximum population in a nest at one time, and the total number of individuals harbored in a nest in a single season.

RELATIONSHIP OF NUMBER OF CELLS IN A NEST TO THE TOTAL POPULATION PRODUCED

Réaumur makes the following statement (p. 170): "Elles [the cells] ne sont pas faites, à proprement parler, pour loger les mouches fort et vigoureuses; chaucune est pour ainsi dire, le berceau d'une guêpe naissante. Quand il n'y auroit que dix mille de ces berceaux, c'en seroit assés pour donner idée du nombreux peuple qui, par la suite, doit composer la petite république surtout quand on sura vû qu'il n'y a peutêtre pas de cellule qui, l'une portant autre, ne serve à élever trois jeunes guêpes."

Crawshay (:05, p. 10) says: "It would be interesting to know to what extent the cells served for a second brood, whether the cells of drones and queens, as well as those of the workers, were occupied."

Observations on this point were made by Ritchie (:15) on the July nest of *Vespa vulgaris* which has already been described above. Ritchie gives the number of workers found as 417, but says that this was not the total number which had been hatched (p. 320). "The number of individuals hatched up to that time is represented by the number of empty cells reoccupied by the members of the second and third broods." In 680 cells, he found stages of a second brood, and in 172 cells, stages of a third brood. He calculates that the total number of workers hatched must have been 852 (in contrast to the 417 found) and concludes that there must have been a high mortality.

Similarly, Isely (:22, p. 342) watched a *Polistes* nest from May 18 until July 10,

during which time 31 cells were built. He says: "All of the cells vacated by maturing wasps were renovated and eggs were again placed in them."

Savin (:25, p. 85) says: "Social wasps are provident in using the cells of their own nest more than once, for after the occupant of a cell emerges as an adult the little compartment is cleaned, and thereby made ready for the reception of another egg."

Parker (:28, p. 14) specifically considers the question of whether or not the white-faced hornet, *Vespula maculata*, rears more than a single young in each cell. Of a November nest, he says: "By taking the topmost comb. . . . and splitting the cells lengthwise with a section razor, I found the cells in the center of this comb had been used several times. Since each larva before transforming to the pupa spins about itself, within the cell, a tough silken cocoon, which remains in the cell after the adult has emerged, the number of these silken cocoons found in a cell indicated the number of larvae that have been reared in that cell. The greatest number found in any cell in this nest was five."

Rau (:29, pp. 669–670) describes a similar criterion for ascertaining the number of times a cell has been used. He says: "The larva feeds during its life and carries the excrement in its body until just prior to pupation, when it ejects it all at one time; since the larva hangs head downward, this material is plastered against the roof by movements of the body, and eventually hardens into a reddish-brown, gritty substance. By counting the number of layers, one can easily tell the number of occupants of a given cell. This same statement applies to certain and probably all species of *Polistes*."

These observations establish, for the species mentioned at least, and probably for other species as well, the fact that the individual cells in a nest may be used more than once in a single season. The reverse of the question, that is, whether all cells constructed are used at least once, is suggested. The only evidence available on this point is derived from Rau's paper (:29, p. 669). He lists the number of cells in the four combs of a nest of *Vespa maculata* as follows:

TIER	OLD OR CAPPED CELLS	TOTAL CELLS
First or top comb	375	375
Second	380	572
Third	54	180
Fourth or lowermost	0	12
	809	1,139

He says: "These figures are of interest in showing that the number of cells built was 1,139, of which 340 [330] were new and too small at the end of the season to have permitted an adult to come to maturity, while 809 cells gave strong evidence of having brought at least one occupant to maturity." Since in this case, all of the cells were not used at least once, and since it has been shown that some cells may be used more than once, it follows that a count of the actual number of cells present in a nest can give no more than a rough indication of the total population produced.

MAXIMUM POPULATION IN A NEST AT ONE TIME, AND THE TOTAL NUMBER OF INDIVIDUALS IN A SEASON

No actual count of a wasp population seems to have exceeded 12,000 individuals, and yet, on the basis of allowing more than one insect to a cell, much higher estimates of the potential capacity of a nest have been made. Réaumur's nest of 10,000 cells he estimated would house 30,000 insects. By a similar computation, his 16,000 celled nest would indicate

a population of 48,000 wasps. The general conception which would be deduced from the life-scheme of a nest, as briefly outlined at the beginning of this paper, is that as more and more workers are hatched, followed by the new queens, and finally the drones, the population steadily increases by constant accumulation until the maximum number is reached, presumably late in the summer just before the first cold weather.

Opposed to this conception is the view that, during a single season, the composition of the community is constantly changing, some wasps dying and others taking their place. On this view an equilibrium would be maintained around a certain population level. The nest would produce in all many more wasps than would ever be found in it at any one time. Wright, Lee and Pearson, (:06, p. 415) state: "It is quite possible that the workers are more heterogeneous than either queens or drones; they may be continuously produced during the season under changing environment." Ritchie (p. 325) says that the life of a worker is limited to three or four weeks, which "may be considered a normal death-rate due to physiological exhaustion at an active period of the nest's existence, in contrast with the much heavier but abnormal death-rate due to external causes, which occurs in late autumn when the colony is hurrying to extermination." It is also to be noted that natural enemies and parasites destroy many individuals.

There is one other observation which may account for occasional very large nests. Isely (:22, p. 343), referring to *Polistes annularis*, says: "On several occasions I have seen a small number—never more than seven—of over-wintering queens renovate a large nest left from the season before and start a colony together. This probably accounts for the very large nests of several hundred cells that frequently are found."

Davis (:19, p. 122) produces evidence on the same point. He says: "It is quite true that each queen *Polistes* usually starts a nest, but sometimes two queens work together." In regard to a twelve-celled nest found in May he writes: "I found two wasps, which I thought at the time had constructed it together, as it was too early in the season for one to be the parent of the other." Of newly started colonies of *Polistes pallipes*, he writes: "Two, three and four wasps were often attending a single nest of four or five cells, and it was evident that the queens had worked together in founding a colony." Further observations on still other nests were as follows:"—three queens with nest of fourteen cells containing eggs, one in each cell; two with nest of ten cells and eggs; one with nest of three cells and eggs; one with nest of seven cells, six of which contained eggs. It will be noted that where there was more than one wasp, that the nests were larger."

POPULATION OF A NEST OF VESPA MACULATA

The following new data are presented as a contribution to the problems which have been discussed above. They are derived from the careful dissection of an exceptionally large nest of *Vespa maculata*. The work was carried out under the direction of Dr. Raymond Pearl.

This nest of the common "black and white" hornet of this region, *Vespa maculata*, was collected by Dr. Pearl on October 22, 1930 from an oak tree on the lawn of the home of Mr. Walter H. Buck in Baltimore County, Maryland. The nest was taken from the tree just in the early evening on a cold day, some hours after all flying had ceased. It may be reasonably inferred that the entire population was in the nest at the time of collection.

Photographs of the nest shown in Figures 1 and 2 were made after the nest had been brought to the laboratory. The nest was measured and preserved in alcohol until October, 1931, when the present study was undertaken.

A sagittal cut was first made, dividing the nest into two halves. The interior

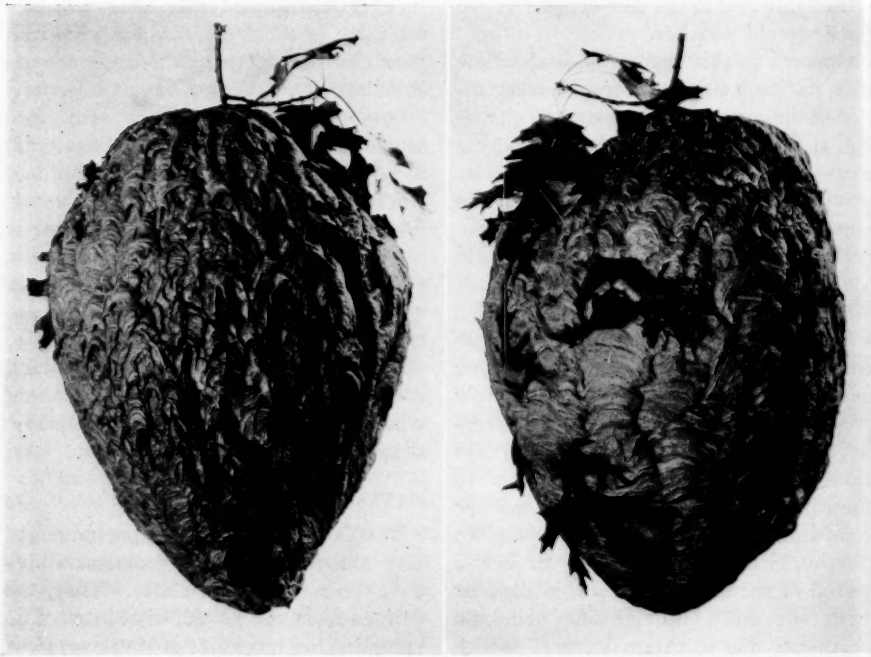

FIG. 1 FIG. 2

FIG. 1. PHOTOGRAPH OF NEST OF VESPA MACULATA (GREATLY REDUCED)
FIG. 2. ANOTHER VIEW OF THE SAME NEST

of one of the halves (Fig. 3) shows the arrangement of the cells in combs. To obtain the population count, the combs were removed one by one and the number of hornets found in the cells noted. To these figures was added the count of those individuals which were loose in the nest between the combs, and those which had fallen from the nest into the bottom of the alcohol jar. The walls of the nest were carefully pulled apart in order to retrieve any insects which might be caught between them. The hornets were then differentiated into four groups: larvae, pupae, adult hornets, and foreign insects (insects other than *Vespa maculata*). The pupae were subdivided according to sex. The adult hornets were separated into three castes: queens, drones and workers.

The various stages and castes are shown in Figure 4.

The dimensions of the nest were:

Greatest circumference = 126 cm.
Height = 46 cm.

Transverse diameter = 40.1 cm.
Volume (computed by formula for an ellipsoid of revolution $2/3\ \pi b^2 l$) = 38,499 cc.)

of paper, were found. The number and distribution of the cells in the combs are shown in Table 2.

The distribution of the population in

FIG. 3. PHOTOGRAPH OF SAGITTAL SECTION OF NEST OF VESPA MACULATA (GREATLY REDUCED)

Five cylindrical combs of cells, built at right angles to the long axis of the nest, and connected to each other by columns

groups is given in Table 3. Larvae and pupae only were found in the cells. Many larvae and pupae, and all of the adults

were found outside of the cells, loose in the nest or in the bottom of the alcohol jar. The larvae and pupae found in the cells were distributed by combs as shown in Table 4. In each case they occupied the peripheral cells.

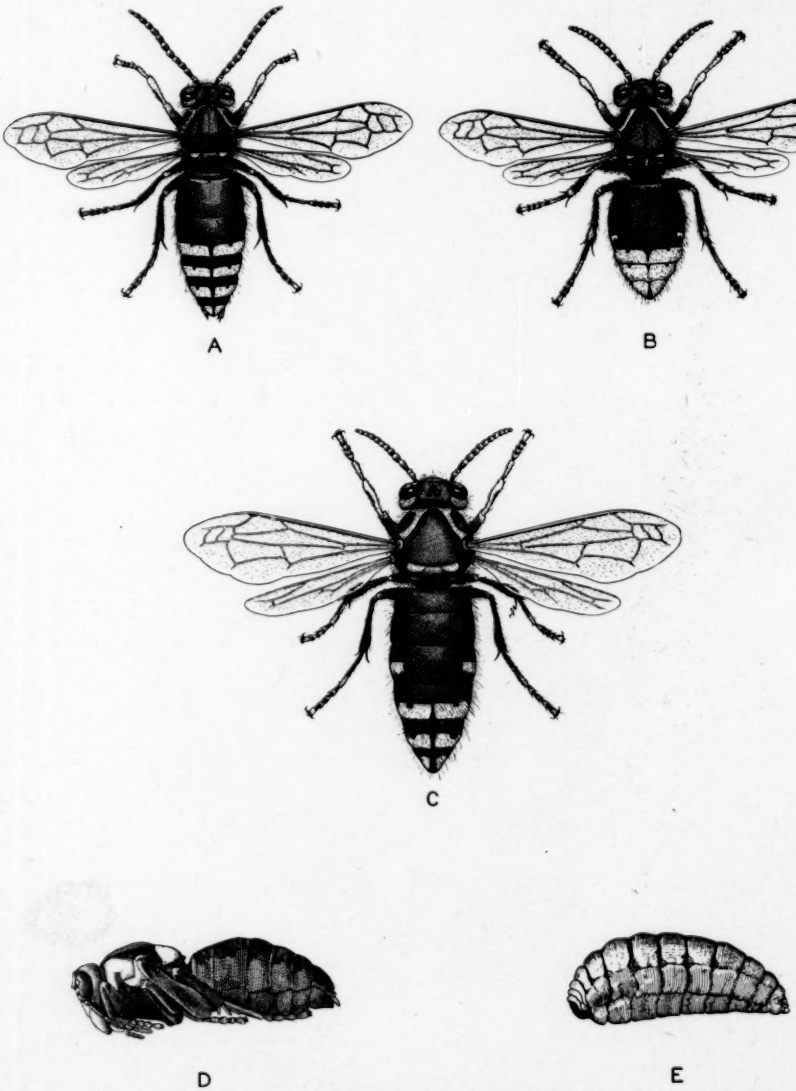

FIG. 4. VESPA MACULATA
A. Drone. B. Worker. C. Queen. D. Pupa. E. Larva

In addition to the vespid population shown in Table 3, there were found in the nest four foreign insects. Dr. Francis M. Root of the Department of Medical Entomology of the School of Hygiene and Public Health, to whom I am very grateful, has kindly identified these as follows:

1. Order Hymenoptera, Family Vespidae.
   Polistes annularis Linn.
2. Order Diptera, Family Muscidae.
   Musca domestica Linn.
3. Order Hemiptera, Family Coreidae.
   Leptocoris trivittatus Say.
4. Order Coleoptera, Family Nitidulidae.
   Glischrochilus fasciatus Oliv.

TABLE 2

Distribution of cells in combs

COMB NO.	NUMBER OF CELLS
1	500
2	530
3	723
4	723
5	719
Total	3,195

TABLE 3

Population of nest of Vespa maculata

	LARVAE	PUPAE		ADULT HORNETS			TOTAL
		Male	Female	Drones	Queens	Workers	
Number	197	68	32	37	51	17	402
Total	197	100		105			402

TABLE 4

Distribution of larvae and pupae in combs

COMB NO.	LARVAE	PUPAE	
		Male	Female
1	39	52	20
2	75	7	4
3	20	2	0
4	0	0	0
5	0	1	1

DISCUSSION

An analysis of the data obtained from this study shows an interesting corroboration of the general pattern which seems to lie behind the development of all species of wasps. Only a rough comparison of size can be made between this nest and those for other species described above. Although the dimensions of this nest are seen to measure up well to the "wirklich kolossal" nest of Oudemans, and to Ormerod's large nest, the number of combs and cells (Table 2) is smaller than for any of the other cases recorded, excepting those of *Polistes*.

This combination of large size and low cell count is probably characteristic of *Vespa maculata*, although the dimensions of nests reported by McCook, Davis, Parker and Rau summarized earlier in this paper are in each case too incomplete to determine this point with certainty. The present nest corresponds closely in size to McCook's, having the same number of combs and only a slightly larger length and diameter. It is considerably shorter than the nest which Davis records, but surpasses Parker's nest in number of combs, and Rau's in number of cells and combs.

In the present case the number of cells was 3,195. Four hundred and two in-

habitants were found, 105 of which were adult hornets. Although it has been pointed out that the number of cells in a nest can give only a rough indication of the total population produced, the difference in this case is too significant to pass unnoticed. The population would seem amazingly small for the size of the nest were it not recalled that the nest was collected late in October, when it presumably had already progressed far in its disintegration. In view of this fact too, more males than females would be expected in the pupal stage, since the drones are the last caste to develop. Réaumur says (p. 199): "Les fémelles et les males paroissent dans chaque guêpier en même temps, et le nombre des unes est à peu-près égal à celui des autres; il y a plusieurs centaines de ceux-ci, et environ autant de centaines de celles-là." However, the counts of Crawshay, Wright, Lee and Pearson, and Pechlauer cited above bear out the observation that a preponderance of males over females is the actual state. In late October also, more queens and drones than workers would be expected. Table 3 indicates the correctness of this assumption.

The four foreign insects found in the nest are all common forms whose habits are well known. They are neither parasites nor commensals. Their presence is presumably purely accidental, or they were carried in as prey. On this point Davis (:19, p. 121) remarks: "*Vespa maculata* is a catcher of flies." Similarly, Bromley (:31, p. 127) observes that "the house fly (*Musca domestica*) is probably the most common prey." The fact that only four are found is interesting. An explanation is suggested by the statement of Comstock (:30, p. 367) that "the social wasps are predacious, and they feed their larvae upon insects which they have malaxated." A hornet's nest would represent a danger zone to all insects other than hornets.

It is a matter of interest that so many more immature hornets were found in the cells of the first two combs than in the others. This observation is in accordance with the statement of Von Wyss (:27, p. 277), that the eggs which are to become queens, and often those which are to become drones, are laid in the first combs of the nest.

(Note: I wish to acknowledge my indebtedness in the preparation of this paper, and to express my gratitude to, Prof. William Morton Wheeler and Dr. J. C. Bequaert for advice and help with the literature, and for reading the paper in manuscript. The illustrations were prepared by Mr. Arthur Johansen, the staff artist of the Department of Biology.)

SUMMARY

In this paper the literature on the nest population in the Vespidae is reviewed and an account is given of the population of a large nest of *Vespa maculata*. Dissection of this nest, obtained in Baltimore County, Maryland, on October 22, 1930, led to the following observations:

1. The nest contained five combs of cells. The individual cells numbered 3,195.

2. The total population of *Vespa maculata* consisted of 402 individuals.

3. In the pupal stage, the number of males exceeded the number of females.

4. In the adult population, queens and drones exceeded the workers in number.

5. The immature hornets were found chiefly in the cells of the first combs.

6. Four foreign insects were found in the nest. Presumably they were accidental invaders, or carried in as prey.

7. The nest, as an organized society, had already undergone considerable disintegration with the approach of cold weather.

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THE UTILIZATION OF WATER COLLOIDS AND MATERIAL IN SOLUTION BY AQUATIC ANIMALS WITH ESPECIAL REFERENCE TO MOSQUITO LARVAE

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INTRODUCTION

THE writings of the German physiologist Pütter (1907–1922) called to the attention of zoologists the possibility of the utilization of dissolved organic material by aquatic organisms. At first his papers raised considerable criticism, which has only subsided within recent years, but in general his theory was treated with marked indifference or contempt. About 1922 certain workers revived interest and began some careful experimental work designed to test the plausibility of such a hypothesis.

Pütter was interested chiefly in marine organisms and concluded that, owing to the tremendous amount of zooplankton and to the paucity of smaller organisms for their food, these animals must be able to derive a major portion of their nutritive elements from dissolved organic material. He also believed that their food is of two types, solid and liquid, and that the latter is absorbed not only by the intestinal tract but directly through the skin. In the case of fish this type of material may be taken up through the gills. Certain aquatic insects were believed capable of the absorption of dissolved substances through their integument. Pütter thought that he had demonstrated the utilization of dissolved food matter by a sponge *Suberites domuncula* and a holothurian *Cucumaria groubei.*

Kerb (1910) states, (author's summary in English),

> To examine the accuracy of Pütter's theory on the nutrition of aquatic animals, I have made experiments on elvers, mingled plankton and *Daphne pulex* which showed throughout negative results:
>
> 1. Grape sugar was not utilized.
>
> 2. Measuring of weight showed that the animals, *Daphne pulex*, are not able to feed on the organic substances dissolved in natural synthesis, but that on the contrary their weight is reduced to a quarter of its former amount.

Moore, Edie, Whitley and Dakin (1912) were among the first to present data in opposition to Pütter. It is "only in the case of fixed animals with slow metabolism such as sponges and ascidians that the demand for organic nutrition can be satisfied by the minute forms present in plankton by such a reasonable volume as may be supposed to be passed through the animal." This was the only confirmation of the proposed hypothesis that they were able to find. On the other hand they criticized Pütter because he based his conclusions on a uniform distribution of plankton which is far from the truth. Utilizing more exact methods for the determination of the organic content of sea-water they believed that they had obtained definite results which placed the maximum at such a low level as to show that the organic content in solution is almost negligible and lies within the limits of error of determination. Perhaps the most strenuous opponent of the

Pütterian hypothesis has been Lipschütz (1913, 1918, etc.), who firmly maintained that aquatic organisms could not derive any nutritional elements from dissolved organic matter. Many other authors have followed Lipschütz in criticising Pütter, but space does not permit a summary of their writings.

PERMEABILITY OF THE INTEGUMENT OF FISH AND AMPHIBIANS

Przylecki (1922) demonstrated that amphibians can absorb organic food through the integument. He first showed that the skin, *in vitro*, was permeable to dextrin, to degradation products of proteins, of amino acids and peptones, and also fatty acids and glycerin. By removing the integument of frogs and making a sack of it he found that these chemicals were able to pass through such a sack. A rather ingenious experiment was designed to demonstrate that living frogs may absorb organic food by the skin route and that these substances can be deposited in the organism (sugar in the liver and muscle) and burned in case of need. The frogs were placed in the particular solution, the anus plugged, and the head kept above the surface. After a suitable interval he was able to demonstrate various sugars in certain organs and tissues (blood, urine, kidneys, liver, etc.). Przylecki (1924), in a comparison of the absorption of the skin and the cells of the intestine as to their special properties distinguishing them from a simple permeable membrane, finds that there is a remarkable agreement, with one notable exception—the permeability of the skin for water in both directions.

Kříženecký (1923, 1925) and Chomkovič (1926) also present evidence in support of Pütter's theory. Chomkovič found that the skin of fish was permeable for solutions of organic materials (glucose, saccharose, peptone). The absorption took place not only through the gills but also through the whole integument.

Adolph (1927) in experimenting with the processes of adaptation in frogs to salt solutions measured the concentration in the tissues of frogs which had been transferred to solutions of sodium chloride of diverse concentrations. He believes there is an exchange of solutes through the skin. Bethe (1929) in considering the permeability of the body surface of sea animals finds that anions as well as cations, especially those contained in sea water, can pass through the surface layers of marine animals. The rapidity with which this occurs varies in different species of animals, which explains the varying degrees of sensitivity to changes in composition of sea water in different species.

Baskervill (1927a, 1927b) tested the influence of sodium and calcium chloride, of dextrose and sucrose on the permeability of frog skin (*in vitro*). He pointed out that urea is an excellent substance for use in considering permeability. It is an organic compound, occurring normally in animal tissues and fluids, penetrates the tissues with moderate ease and in amount sufficient for accurate quantitative determination. He found that the permeability of the skin is decidedly influenced by the electrolytes and non-electrolytes in contact with the tissues.

Taniguchi (1929) in a paper on the feeding of amphibian larvae with various nutritive materials (solutions of both organic and inorganic substances—ferrosomatose, hemaglobin, iron sulphate, sodium chloride, sodium bromide, sodium iodide and beef liver powder) demonstrated that the tadpoles could utilize certain of these compounds for nourishment. The abstract of this paper does not state whether absorption is believed to take place through the skin or directly through the alimentary tract. It is prob-

ably through the latter, since tadpoles are truly aquatic and must take in a certain amount of water with their food, whereas adult frogs feed either at the surface or out of water.

All of these workers have experimented with materials in true solution which will dialyse through a semi-permeable membrane. If the skin of the animal is permeable it might be expected that such substances would be taken up. On the other hand colloidal matter in suspension would probably not be capable of penetrating the uninjured skin.

THE UTILIZATION OF MATERIAL IN SOLUTION BY MOSQUITO LARVAE

Matheson and Hinman (1929, 1930) suggested that much of the food of mosquito larvae might be obtained from organic substances in solution. This suggestion was based on observations of the plankton of breeding and non-breeding pools. Their results indicated that in at least certain cases the phyto- and zooplankton were more varied and abundant in pools in which no larvae were found than they were in those containing larvae. This might be expected when it is recalled that many species of mosquitoes breed in pools of a very temporary nature—especially those early spring woodland species which are found in collections of water formed from melting snows and could not possibly have an opportunity to contain as much plankton as permanent pools. Experimental evidence in support of this hypothesis was given by the writer (Hinman, 1930), who demonstrated that the larvae of *Aedes aegypti*, the Yellow Fever mosquito, may utilize material in solution and colloids in suspension. Using water abnormally rich in such substances, he was able to rear certain larvae through to maturity in Berkefeld filtered water. The eggs were disinfected before introduction and the media proved sterile at the end of the experiment. In these cases the entire growth was made on either material in solution or in suspension. In fact many of the colloids with larger molecules were probably taken out by the filter along with the organisms. Matheson and Hinman (1931) have utilized a similar technique for other culicines.

Howland (1930) found that certain vital stains such as neutral red and methylene blue were rapidly absorbed by the cells of the intestinal tract (Aedes spp.). So readily were these stains taken up that red and blue larvae were seen swimming about in liquid of the same color. The author suggests that the pond water and contents of algae in solution might well be taken up in a similar manner. Shipitzina (1930) demonstrated the presence of a mechanism by which the larvae (*Anopheles maculipennis*) are able to filter out and concentrate colloids from water. Using solutions of Chinese ink (containing particles from 1μ to 0.1μ), carmine, and a medical preparation of colloidal silver (the diameter of the particles of which was $20\mu\mu$) he found that the concentration of the substances tested was much greater in their alimentary canals than in the water. Evidently the larvae were unable to filter particles of which the diameter was as small as $5\mu\mu$ (soluble starch). These chemical proofs of the ability of larvae to filter and absorb colloidal material form a noteworthy advance in the study of the nutrition of mosquito larvae.

The writer (Hinman, 1932) has shown that anopheline larvae are able to utilize material in solution and colloids in suspension from normal pool water. This has been demonstrated by the use of Seitz-Werke filtered media and the dialysable and non-dialysable fractions of water from breeding habitats. Larvae of *Ano-*

pheles crucians and *A. quadrimaculatus* were found to live up to maximum period of 11 days and to make a significant growth, under sterile conditions, in Seitz-Werke filtered water. The larvae were unable to develop in the dialysate of water from the same breeding pools. In the case of the non-dialysable fraction of the same type of water it was found that the larvae may live up to 6 days and make a slight growth, indicating that the normal water colloids may be of considerable importance in their nutrition. A calculation of the filtration capacity of anopheline larvae based on the findings of Senior-White (1928a) and Shipitzina (1930) show that the larvae are able to filter *per diem* a volume of water from 20–200 times their own volume.

Unpublished data demonstrate that *Aedes aegypti*, *Culex quinquefasciatus* and *C. salinarius* also derive nutritive elements from the non-dialysable fraction of water from their breeding pools. Work in progress shows that culicid larvae are able to subsist for a period, in certain cases exceeding 10 days, in various amino acids and carbohydrates (C.P.) dissolved in Ringer's solution or physiological saline, under sterile conditions. While no recognizable growth was made yet the fact that these larvae were able to remain alive and active for such long periods clearly indicates that they must derive nourishment from these substances.

In connection with this problem it is interesting to note certain papers dealing with the ecology of culicid larvae. Williamson (1928) believes that it is not improbable that changes occurring in the katabolic arc of the nitrogen cycle, in relation to more or less complete oxidation of proteins and their derivatives, are an important, if not the chief chemical cause determining the suitability of water for anopheline larvae as a whole, as well as that of particular types of water for those particular species. MacGregor (1927), Beattie and Howland (1929) and Van Thiel (1928) suggested that culicid larvae are able to secure nutritional elements from decomposing vegetable material and detritus. Rudolfs and Lackey (1929) concluded that specific substances either present in the water or produced by the decomposition of vegetable matter may be responsible for the growth of micro-organisms and subsequently for the breeding of mosquitoes. Sebéntzov and Adowa (1929) conclude that there is almost no natural water that does not contain nourishment for larvae even when microscopical examination reveals a scarcity of organisms—that there exist food substances difficult to disclose ("ultra-seston détritus autochtone extrêmement fin"). Harvey and Symes (1931) found that samples of water containing anophelines show higher organic indices than those without larvae.

There is no reason to believe that Culicid larvae are the only aquatic insects which are able to utilize dissolved materials. Wu (1931) in experimenting with *Simulium vittatum* found that larvae ranging from medium to mature stages continued to develop and transform to adults when the plankton in the water was almost, if not completely, removed by passage through a sand filter (12 inches of sand). Evidently these larvae were able to derive nutritional elements from either the dissolved material or the colloids of flowing water.

ORGANIC CONTENT OF NATURAL WATERS

A group of workers from the University of Wisconsin have considered the organic content of natural (fresh) water in some detail. Birge and Juday (1922, 1926a, 1926b) in an investigation of the chemical composition of the water of certain lakes

in Wisconsin found that Lake Mendota contains more than nine times as much soluble nitrogen as the total plankton nitrogen.

> Considering the proteins alone there is dissolved in the water three times as much as exists in the plankton. These soluble proteins are as nutritious, so far as their chemistry gives evidence, as are those of the plankton. Outside of the centrifugable plankton there is four times as much ether extract as within it and the organic carbon assignable to carbohydrates is ten times as great outside the plankton as within it.

Domogalla, Juday and Peterson (1925) in determining the forms of nitrogen in certain waters found that "the seasonal variation in the different forms of soluble nitrogen indicates that these compounds form part of the nutriment of both plant and animal life." They consider that the origin of the soluble nitrogenous compounds may be attributed to the inflowing water, the decomposition of the insoluble organic material, and the excretion of aquatic animals. Peterson, Fred and Domogalla (1925) established both by qualitative and quantitative methods the presence of proteins and amino acids in lake waters. Among the amino acids found were tryptophane, tyrosine, histidine, arginine, and cystine. They maintain that the type and quantity of plankton growing in a lake are important factors in determining the quality and quantity of amino acids in solution.

These results are somewhat opposed to those of Moore, Edie, Whitley and Dakin, mentioned above. However, it must be remembered that the latter workers examined sea water and this might be predicted to be relatively low in organic matter in solution as compared with bodies of fresh waters. Lakes in general might be expected to be less rich in such materials than smaller pools, especially those with large quantities of vegetable and animal decomposition.

A number of investigators have analysed the waters of breeding pools of Culicidae in order to determine whether any chemical factors were responsible for the distribution of breeding. Notable among these are Senior-White (1926, 1928b) in Ceylon and India and Rudolfs and Lackey (1929) in the United States. The former found that saline ammonia in amounts exceeding 1 part per million is inhibitory to the natural-water breeders, especially anophelines. Rudolfs and Lackey, however, observed that *Aedes canadensis* larvae were present in pools "with either high or low total acidity, with free CO_2 of 4 and 40 p.p.m., with 5 and 75 p.p.m. chloride, with 2 and 112 p.p.m. sulfates, with high and low acid carbonates, free ammonia and albuminoid ammonia. The carbon-nitrogen ratio did not seem to affect breeding." From these and other investigations there seems to be little correlation between the various chemical constituents, as limiting factors, and larval breeding, the single exception being saline ammonia (Senior-White).

DISCUSSION

It is a well known fact that bacteria and certain protozoa are able to subsist on inorganic and organic materials in solution and may be cultured on synthetic media. Metazoa in general have specialized tissues to elaborate highly active chemical substances for the breaking down of complex organic materials into smaller molecules and bringing them into solution so that they may be readily absorbed. These soluble fractions are then used as building-stones from which the more complex materials are synthesized and metabolized. It would seem only reasonable to believe that, should

any of these basic substances occur in the water and find their way into the intestine, the organism would be able to utilize them. As mentioned above certain amino acids (including three of the so-called essential ones: tryptophane, cystine, histidine and lysine, which the human body is unable to elaborate) are known to occur in lake waters.

The demonstration of the absorption of these materials is an exceedingly difficult and tedious matter. Their existence in the water has been shown, but only by such methods as filtration, dialysis, etc., can their utilization be proven. Unfortunately such experiments, to be conclusive, must be done under sterile conditions to eliminate the effect of micro-organisms. As yet only a beginning has been made on this vast problem, which is of such fundamental importance to our theories of the nutrition of aquatic animals.

MECHANISM OF ABSORPTION

Whether certain aquatic animals as fish and amphibians *normally* absorb any considerable amount of organic material through the skin is probably still debatable. Przylecki, Chomkovič, etc., have demonstrated that, under experimental conditions, specific organic compounds may be taken up. If these substances exist in the water in sufficient concentrations there is no reason to doubt their penetration through the skin of those animals which have an integument, semipermeable in nature. The ability of non-dialysable materials to pass through the uninjured skin certainly requires demonstration. Aquatic insects and other arthropods, with a chitinous exoskeleton, would probably be unable normally to absorb such material. In the case of mosquito larvae and other water-filtering animals considerable fluid must pass into the intestine. (Howland (1930) found that when mosquito larvae were actively feeding the whole contents of the gut might be replaced in from 10–15 minutes). If nutritional elements are contained in this liquid why should they not be absorbed by the cells of the intestine? Direct absorption of material in solution would seem to be a much simpler process than the employment of digestive juices to first break down the complex materials and prepare them for absorption. As yet no work has been published on the enzymes present in the intestine of Culicid larvae, but a beginning has already been made on this problem. Even if proteolytic, lipolytic or carbohydrate-splitting ferments are found present in the gut this would not preclude the possibility of absorption of certain organic compounds such as amino acids, monosaccharides, etc., already existing in the water and which do not require enzymatic action. (See Hinman, E. H. Enzymes in the alimentary canal of mosquito larvae. Preliminary communication. Proc. Soc. Exp. Biol. Med., *29:* (in press). 1932.)

SUMMARY

A review of part of the vast literature on the question of the utilization of material in solution by aquatic animals, as suggested by Pütter and others, is presented. Special reference is made to mosquito larvae and evidence for the utilization of material in true solution as well as colloids by both anophelines and culicines is submitted. A discussion of absorption through the skin is given and the organic content of natural waters is considered. Brief mention of the mechanism of absorption of such material is made. It would seem that many aquatic animals probably utilize organic material

in true solution as well as water colloids but this method should be regarded as supplementary to other feeding habits,—in other words our theories of nutrition of aquatic animals might well assume a position midway between the extremes of Pütter on the one hand and of Lipschütz on the other. Direct absorption of these materials by the cells of the intestine is believed to be of greater importance, in at least the case of mosquito larvae, than cutaneous absorption.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

THE GEOLOGICAL-AGES HOAX. *A Plea for Logic in Theoretical Geology.* *By George McC. Price. Fleming H. Revell Co., New York.* $1.25. 5 x 7½; 126; 1931.

Once more THE QUARTERLY REVIEW OF BIOLOGY is able to introduce a distinguished Fundamentalist to its readers, and to add to its *Fundamentalist Portrait Gallery*. We take great pleasure in presenting George McCready Price, B.A., M.A., head of the departments of geology and philosophy at Emmanuel Missionary College, Berrien Springs, Michigan; formerly professor of English literature at Fernando Academy, of chemistry and physics at Lodi Academy, and of geology at Union College, California and at Union College, Nebraska; and author of "God's Two Books," "The Fundamentals of Geology," "Back to the Bible," "The New Geology, a Textbook for Colleges," etc., etc. We have heard Professor Price referred to as "Bryan's geologist," but we hope to make it clear that this is entirely inaccurate. Professor Price has no good word to say for the reconcilers of Genesis and geology.

Today the Fundamentalist who accepts all these successive sets of animals and plants in a progressive series, and who still stands out against the theory of evolution somehow, proves that he has never thought this matter through to the end. He is simply repeating parrot-like some things which he learned in college perhaps two generations ago, when these successive creations and catastrophes were commonly taught, and when good, sincerely Christian men like Dana, Hugh Miller, Hitchcock, Gladstone, George Frederick Wright, and many others tried to "harmonize" these successive geological "ages" with the days of Creation, as mentioned in the first chapter of the Bible.

The history of the past fifty years or so is abundantly proving that all this was a big mistake. The Christian Church has lost out in every single instance where she has tried to keep up this line of argument. A conspicuous example of this utter failure was the piteous figure which poor W. J. Bryan made in the hands of a skilful evolutionist like Darrow. If the tables had been completely turned, if Darrow had been put on the witness stand, and if some one who understood geology had asked him to prove the scientific reality of his successive "ages," the results might not have contributed so much to the gaiety of the nations, but they would have done vastly more for the enlightenment of mankind in this work-a-day world of realities. Bryan and his supporters did not realize that they had already travelled far on the road to organic evolution by admitting these unproved and unprovable geological "ages," and that they must always make themselves ridiculous by trying to stop at the mere origin of species. Such tactics must always end in defeat and humiliation. The present crestfallen attitude of the Evangelical Church is almost entirely due to the fact that for over half a century this day-ages theory has been the only "recognized" apologetics of the Christian Church.

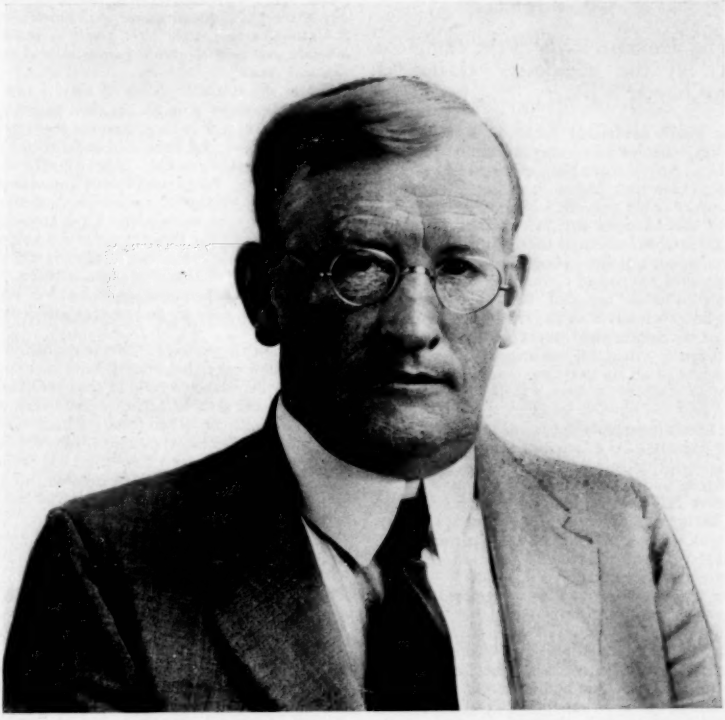
Professor Price falls into no such mistake as this. No distortion of Holy Writ enters his scheme; he is out to prove that the rocks are consistent with an honest reading which assumes that the Bible really means what it says. He begins by showing that there is real evidence of a catastrophe at no remote date. He points first to the "visible evidences [in Wyoming] almost everywhere of a vast mass of water as it stood here for a short time, forming real sea-beaches still so

clearly marked, or was gradually drained from off these lands. And this vast mass of water must have been here at no very remote period." . . . . "These phenomena . . . . are eloquent testimony of the time when the lands were emerging from beneath the waters, or when the oceans were retreating away to their present reservoirs, where they still obey their orders, 'Thus far and no further.' "

The fact that the great mountain ranges are of geologically recent origin reinforces the catastrophic evidence; and when we observe that fossils of tropical animals and plants are found in northern regions, we are obliged to conclude that "the ancient world was characterized by a most wonderful climate, in which delightfully mild conditions prevailed over all the polar latitudes. This climate of spring-like loveliness apparently was continuous and uninterrupted until that fatal and sudden change (whatever its cause) which put the vast herds of elephants into cold storage, the evidence for which is familiar to every intelligent person."

The Professor now proceeds to expose in all their nakedness the presumptuous and misguided geologists who pretend to be able to distinguish a time sequence in the rocks. He points out that

> it is self-evident that no fact or set of facts known to us can justify us in saying that the kinds of life which we call Cambrian must have actually lived and died before those which we class as Carboniferous, or that the Cretaceous forms of life lived before the Tertiary, or indeed any of these before any of the others. Unless we are to dispense with strict logic in these first steps of the science of geology, we must admit that we can never prove the Cambrian forms of life to be a day older than all those many other groups which are often found in the same bottom position.



GEORGE MCC. PRICE

Further, the sequence of the geologist has gaps; beds which the geologist says are millions of years apart are actually found in perfect order, with no trace of the beds which should separate them;

splendid proof that the many millions of years alleged to have passed between the laying down of the two successive beds concerned never really existed at all in these localities; and if they did not exist here, the implication is clear that the missing floras and faunas do not represent real "ages" anywhere else.

The final demonstration of the complete absurdity of the geologists' claims is given in Chapter VII.

Are the fossils invariably found in a certain sequence? Yes,—except when they are found in some other order. An "invariable" order with almost innumerable exceptions, and with plenty of examples of almost every other possible order, is it? What a travesty on the scientific method to take this supposedly "invariable" order of the fossils as an absolutely certain index of the various successive "ages" which our world has passed through. And yet some clergymen who loudly boast of their "Fundamentalism" now find themselves in the stultifying position of trying to harmonize the "days" of Creation mentioned in Genesis with this traditional order of the fossils, in spite of all its twistings and uncertainties. One might as well try to steer a ship by a weathercock.

Do the fossils invariably occur in the same relative sequence? Hardly. As Marius R. Campbell, Principal Geologist of the United States Geological Survey, recently wrote in a letter to the present writer, the reverse order is "now recognized as being of very common occurrence, but because of the apparent conformity of the overthrust mass and the rocks upon which it rests, such phenomena have in the past been incorrectly interpreted."

In other words, those who first examined and reported on these various cases thought that the rocks had of course been laid down as we find them. But afterwards, when it was discovered that the fossils were in the reverse of the "invariable" order, they and their fellow geologists had to invent the theory of thrust faults, and say that the "older" rocks must have been lifted up and pushed over bodily on top of the much "younger" ones, in order to save the reputation of their "invariable" world time-table.

Some of Professor Price's answers to hypothetical objections to his views are worth quoting.

1. *A world-catastrophe, such as you have postulated, would involve a real miracle; perhaps a series of miracles; and it is not scientific to invoke a miracle to explain a difficulty in natural science.*

I suppose that something very much like a miracle is involved in the events alluded to in the preceding pages as an explanation of how the ancient world was transformed into the present one. But what is meant by a "miracle?" As a believer in the living God, Who is still in charge of His universe (not an absentee God) and Who presumably can do as He sees fit with anything or everything in it, I do not take any great interest in the distinctions which some people try to make between natural laws and miracles. . . . We live in a world of constant "miracles;" and instead of diminishing in number they seem to be increasing with the progress of our knowledge of science. One more "miracle" added to the list should not disturb us in our scientific thinking. . . .

3. *We own that the seeds of plants and possibly the eggs of fishes and of the invertebrate animals might possibly survive such a world-convulsion as you invoke; but how could any of the land animals escape destruction? How could a big wooden boat, with eight people in it and a lot of animals, ever come through a year or more of such turmoil safe and sound?*

From the scientific point of view I can only say that, while many animals perished entirely, so as to leave no survivors, a large number seem to have survived somehow; for their descendants are still with us in the modern world. Scientifically there is no other answer. What does *science* know about any of these things? From the point of view of the Bible record, it is easy to say that God attended to this matter of caring for those types of animals which He wished to survive, so as not to destroy the life of the globe entirely. I know no other answer.

4. *If man was contemporary with the trilobites and the dinosaurs, why don't we find some human skeletons among these fossils?*

Although this sounds like a scientific objection, it can be answered either scientifically or theologically. The scientific answer would be that probably man did not live in the same localities as the trilobites and the dinosaurs and the other fossils which we have discovered. . . . The theological answer would be that, since we are told that the Lord wished to destroy that ungodly race, He probably did a good job of it and buried them so deeply that we have not yet discovered their remains. . . .

7. *Why do we find the various kinds of plants and the various kinds of animals all so segregated off by themselves, as we seem to find them in the strata?*

It is possible that, in the earth as the Lord first made it, the floras and faunas would be much more sharply defined than at present. An artistic gardener plants his various flowers largely in beds by themselves. . . .

15. *On the theory of a great world-catastrophe, how do you explain the presence in Australia of all the chief marsupials (except the opossums), and in South America of all the chief edentates?*

I confess that the distribution of the great groups of animals is a very difficult problem. But I do not think it is more of a problem for me than for the evolutionist. The latter has to say that the mammals, in all probability, spread out from some centre in Western Asia. On the theory of a universal Deluge, according to the record in Genesis, this same place became the distributing point from which all the surviving animals must have scattered out over the face of the recently desolated earth. But on my view, these animals were under the direct guidance of the Creator, Whose care is still marvellously shown in the migration instincts of such animals as the storks, the swallows, the golden pheasants, the Arctic terns, and the eels. On the basis of the evolutionary theory, what is there except blind chance to

superintend the distribution from the common centre which we know actually occurred?

We may perhaps close with a last quotation, with which we can whole-heartedly agree:

When any Christian takes this position, intelligently informing himself regarding the pertinent facts as now known, he finds himself in a position where he has a safe and secure line of argument. And from this position he can never be driven by any fact or set of facts known to the modern world.

MAN'S OWN SHOW: CIVILIZATION.
By George A. Dorsey. Harper and Bros., New York. $5.00. $6\frac{1}{4}$ x $9\frac{3}{8}$; xvi + 977; 1931.

In this, his last book, the author of *Why We Behave Like Human Beings* gives a panoramic view of man's life from the earliest times as a member of organized society. In contrast to the politically minded historians of the orthodox school, his viewpoint is biological.

Man had to take to culture or perish: no man-made additions to man's natural endowment, no human race. Culture was a biologic necessity. As culture helped man to supply his physiologic needs, it had survival value in the struggle for existence—and still has. As culture was man's answer to the will-to-live, civilization is his answer to the will-to-power, to live more, bigger, faster, longer, for ever: insurance for life and immortal glory in death; to live like a magician, like a king, like a demon, like a god.

In the first part he considers the basic factors on which culture is built up: language; sexual behavior, especially the tendency to form continuing sexual partnerships; the esthetic sense; and religion, the impulse to fear and propitiate the unknown. Having laid his foundation he traces the development of civilization through the long ages of prehistory, through Egypt, Babylonia, Assyria, and Persia, to its flowering in Greece. But the glory that was Greece decayed after a few centuries and on its ruins was enthroned Christianity, a mystery religion, based upon the myths of the East.

Our familiar spirit whispers in our ear that the book is certain to be placed on the *Index*. McCabe himself could not roast the Holy Catholic and Apostolic Roman Church to a neater brown. But Dorsey plays no favorites. Having demonstrated the pernicious effect of the Roman Catholic system on civilization, he turns to show that the Reformers were fundamentally as irrational and illiberal as the cult against which they protested. Luther denounced Copernicus; Calvin burned Servetus. The revival of science in the sixteenth and seventeenth centuries owed as little to the Reformers and the Humanists as to the medieval Church. For our knowledge of Greek science we are indebted mainly to the Arabs. The development of the physical and biological sciences during the last three centuries, on which our modern civilization is based, was not fostered by any Church, but had, on the contrary, to struggle forward against the weight of ecclesiastical opposition.

Having traced the past development of civilization, Dorsey leads us up to Pisgah and shows us the Promised Land where supernaturalism and intellectualism, asceticism and nationalism, shall be no more, where science shall set up the reign of social justice, free humanity from the scourges of disease and ruling classes, and lead our feet into the way of peace. And then he calls on the clergy, of all men in the world, to lead us over Jordan! He might have chosen likelier guides.

THE ADVENTURE OF MANKIND.
By Eugen Georg. Translated from the German by Robert Bek-Gran. E. P. Dutton and Co., New York. $5.00. $5\frac{1}{2}$ x 8; 325; 1931.

One might expect a German outline of history to be sober, solid, exhaustive, and perhaps a trifle exhausting. On the contrary this is the dizziest book we have read in several blue moons. The *Ur-Homo*, it seems, appeared in the Mesozoic or earlier.

He was a mammal or a marsupial, with a suddenly rising intelligence. He walked upright on his hind-legs and on the soles of his feet; he had five fingers. But his gigantic form was undoubtedly bizarre and grotesque, for his body expressed the "age signatures" of the amphibian world which ruled triumphant through the declining Paleozoic Age.

He had the dragging walk of the amphibians, and between his fingers and toes were webs. His hand was still undeveloped; his thumb had not yet come

to stand opposed to the fingers. The head was pointed and dolichocephalic, and perhaps this man had in common with the other amphibians and reptiles—and like Achilles and Siegfried—"a body partly armored with horn, which trait can be considered a characteristic of the declining Paleozoic Age, since the amphibians were increasingly transforming their slimy skin, covered with glands, into such an armor, most frequently on the throat and breast but also on the back, and were strengthening it with scales. This hypothetical man also possessed a well developed parietal organ, an eye-shaped opening in his skull which was common to all those earliest land inhabitants and can surely be considered as an undefinable yet somehow ocular sense organ." (Dacqué) . . . .

Thus, this original *Homo*, who was first considered only a hypothetical, reptilian man-ape, is shown to have existed! Around the year 1000 B.C. he still lived on Earth (perhaps as a race). The fact that an Assyrian artist cut his shape upon the rocks is unshakable proof of his existence. For those Assyrian half-men are real: they actually lived, and were realistically depicted in all the details of their environment. Nor can one dismiss them as stylized conceptions of fantasy. The accompanying texts of the reliefs state that the captured beast-men are being led by soldiers; and again, that these strange beings are part of the tribute which the land of Musri (Aramaic territory) is sending to the king; and that these *baziati*, *udumi*, *pagutu*—these beast-men, in fact—appear among the other gifts of tribute as especially important items.

From this primitive species were evolved the giants and pigmies, and much later—a scant four million years ago—*Homo sapiens* appeared on the continent of Atlantis and developed a high civilization. This explains the many puzzling parallelisms between the cultures of the Eastern and Western hemispheres.

However, there are gleams of sanity in the book, as, for instance, in the following note on science: "With his senses man observes the phenomenal world, behind which something is hidden which his senses cannot see. Their limitations are the boundaries of his knowledge. He believes he knows. He believes he is exact. But in reality he only shifts the riddle and actually covers it up. To do so he has invented the game of nomenclature. He puts a scientific label on a riddle and calls it solved!" It is true that science deals with phenomena and their observed correlations and that scientists sometimes delude themselves with the belief that, because they have labeled a phenomenon, therefore they understand it. But when Herr Georg goes on to argue that by metascientific means—magic and the like—one may penetrate to a knowledge of noumena, we fail to follow him. As a romance the book is amusing enough, but why a respectable publisher should tout it as a serious contribution to thought is beyond us.

THE ORIGIN AND NATURE OF MAN. *An Enquiry into Fundamentals. Reconciling Man's Proud Achievements with Man's Humble Descent.*

By G. Spiller. Williams and Norgate, London. 15 shillings net. 6 x $9\frac{5}{8}$; xiv + 383; 1931.

This is an interesting, extensively documented, and rather learned book. Its main thesis, and its general point of view, however, are so far contrary to the prevailing and orthodox dogmas of the sciences and near-sciences having to do with man that it has no chance of a sympathetic hearing and but little of ever being read at all. Mr. Spiller is a latter-day and more biological Buckle. This, of course, cooks his goose with the scientifically righteous. His contention is that man's physical relation to the animal world is close and evident. Mentally he is enormously remote from it. Furthermore there are among men tremendous *observable* differences in mental status, but no noteworthy differences in *innate* mental status. In short the position is that the observed differences between men in respect of their mental equipment are due to nurture [opportunity, education, "time-binding"], phylogenetic as well as ontogenetic, rather than to nature.

Like all crusaders Mr. Spiller overstates his case. Like most of them he is more earnest than seductive in his literary style. But, all in all, the book has in it a good deal of learning and some wisdom.

DIFFICULTIES OF THE EVOLUTION THEORY. *By Douglas Dewar. Longmans, Green and Co., New York.* $5.00. $5\frac{1}{2}$ x $8\frac{1}{2}$; viii + 192; 1931.

The author holds that, while evolution

may be admitted to have transformed certain forms, these are of a relatively minor nature, and that there is not merely no evidence for the occurrence of more important transformations, but that there is much to show that such transformations did not in fact occur. The book shows a considerably greater acquaintance with the facts of zoology than most of the anti-evolution literature. It might be useful to the convinced evolutionist to consider seriously how some of the arguments should be answered. It would be an exercise in logic, if nothing more. It should be pointed out that the author is a trained naturalist, with long, varied, and extensive field experience.

THE VERTEBRATE FAUNAS OF THE ENGLISH EOCENE. *Vol. I. From the Thanet Sands to the Basement Bed of the London Clay.*

By Errol I. White. With Appendices on the Otoliths, by G. A. Frost; and on the Mollusca, by Arthur Wrigley. The British Museum (Natural History), London. 10 shillings net. 5½ x 8½; xiv + 121; 1931.

The first volume of an account of the vertebrate fauna from the lowest Eocene beds between the Chalk and the London Clay. Most of the fossils described are remains of fishes, which, with the exception of a white bream from the Woolwich beds of Croyden, consist of isolated teeth, bones and skin tubercles. Dr. White gives not only a description of new species but also a detailed revision of previous records. There are numerous drawings and an index.

BIOLOGY AND MANKIND.

By S. A. McDowell. The Macmillan Co., New York. $2.50. 5¼ x 7½; xviii + 229; 1931.

A short account of evolution and heredity, based on a portion of a course given to senior boys on the classical side at Winchester College. (English school boys must somehow accomplish more than American). It is well done, on the whole, though there are many points of detail which will annoy or enrage the specialist; some of them matters of opinion, and fewer of them matters of fact.

GENETICS

CANCER AND RACE. *A Study of the Incidence of Cancer Among Jews. Conducted under the auspices of the Jewish Health Organisation of Great Britain.*

By Maurice Sorsby. William Wood and Co., New York. $3.00 net. 5¼ x 8¼; xvi + 120; 1931.

On the basis of statistics from London, Amsterdam, Vienna, Budapest, Warsaw, Lodz and Leningrad, Dr. Sorsby finds that there is a closer relationship between the cancer mortality of Jews and non-Jews of any particular city than between that of the Jews of different cities. However, the incidence of cancer according to site differs among the Jews and their Gentile neighbors. This difference, he concludes, can be satisfactorily explained for most sites by established etiological factors; for example, the absence of cancer of the penis among Jews is probably due to the custom of circumcision, since Mohammedans of diverse racial origin share this immunity. For the high incidence of intestinal cancer among Jews he finds no such specific factor but concludes that the great variations observed for the Jews of the different cities make a hypothetical racial factor quite unsatisfactory as an explanation. There are a bibliography of 42 titles and an index.

HUMAN HEREDITY.

By Erwin Baur, Eugen Fischer, and Fritz Lenz. Translated by Eden and Cedar Paul. The Macmillan Co., New York. $8.00. 6 x 9¼; 734; 1931.

This is a translation of the third (1927) edition of the authors' well-known German text, which has been noticed in these columns (Vol. III, p. 136). "Supplements and corrections" were supplied for the English edition during the translation, but in the main it corresponds to the German edition noted. It is an advantage

to have this standard work available in English.

VERHANDLUNGEN DES I. INTERNATIONALEN KANINCHENZÜCHTER-KONGRESSES, LEIPZIG 1930.

Edited by Hans Nachtsheim. Reichs-Zentrale für Pelztier- und Rauchwaren-Forschung, Leipzig. 28 marks. 6¼ x 9¼; ix + 284 + 7 plates; 1931 (paper).

Although many of the papers in these Proceedings are written from the standpoint of the commercial rabbit breeder, there are a number which will interest the biologist. Among these are papers by Hammond on the processes of reproduction in the rabbit; by Hagedoorn on crosses between the rabbit and the hare; by Nachtsheim on the Rex rabbit and on hereditary syringomyelia; by Pease on the inheritance of yellow fat in rabbits; by Sprehn on parasites of rabbits; and by Meyn on infectious diseases of rabbits.

CURIOSITIES OF HEREDITY. *"The Hapsburg Lip."*

By J. N. Ungoed Pitt. John Bale, Sons and Danielsson, London. 2 shillings net. 4¾ x 7¼; 25; 1931.

This little book is dedicated to Dr. Leonard Portal Mark, an acromegalic, who at the time of his death left a remarkable series of observations based upon his own experiences with this distressing disease, together with numerous records concerning the occurrence of this and similar deformities in man. It consists of a grouping together of some of these notes, particularly those dealing with the Hapsburg lip, which Doctor Mark followed through many generations. He found no support to the theory which has sometimes been advanced that the Hapsburg lip is in any way connected with acromegaly.

MENDELISM AND EVOLUTION. *The Dial Press Monographs on Biological Subjects.*

By E. B. Ford. The Dial Press, New York. $1.50. 4 x 6½; xii + 116; 1931.

A brief but clear and illuminating discussion of the relation of modern genetic knowledge to the problem of evolution. We recommend it to biologists who may have been appalled or even overwhelmed by the mathematical discussions of Fisher, Haldane, and Wright. They may find the statement of what the equations are all about more clearly put here than in some of the more formally learned papers.

GENETIK UND EVOLUTIONSPROBLEM.

By Victor Jollos. Akademische Verlagsgesellschaft, Leipzig. 3 marks. 6 x 9; 44; 1931 (paper).

An attempt to show that cases of orthogenetic evolution can be explained by directed mutations, such as Jollos and Goldschmidt have found in *Drosophila.*

GENERAL BIOLOGY

THE PHILOSOPHICAL BASIS OF BIOLOGY. *Donnellan Lectures, University of Dublin, 1930.*

By J. S. Haldane. Hodder and Stoughton, London. 7s. 6d. net. 5¼ x 8⅜; x + 169; 1931.

In these days of extensive research when maximum output appears to be the ideal among biologists and relatively little attention is given to the establishment of fundamental concepts of biology, this book comes as a welcome contribution.

The author aims to discuss as clearly as possible the theoretical aspect of biology which his observations have forced upon him. In the first lecture we are introduced to the fundamental axiom of biology, which is that life is the "coordinated maintenance of structure and activity," or, "The life of an organism is an indivisible coordinated whole which is constantly maintaining and reproducing itself." Biology is just as exact a science as those to which mathematical theory is closely applied. The difference lies only in the fact that the latter assume in their axioms a separability in space whereas "insofar as we discover co-ordinated maintenance we reach exactitude in biology."

Haldane discards the mechanistic phi-

losophy because a physico-chemical explanation of living phenomena cannot be exact or real, and the vitalistic philosophy because it neglects to consider adequately the interaction between environment, both external and internal, and the organism.

In the second lecture Dr. Haldane illustrates his main thesis by a technical account of the mechanism of respiration. We see that a pure physico-chemical explanation is unintelligible, and that the phenomena can only be explained by considering the organism as a whole. "The whole which is studied is, moreover, no mere aggregate of separable units and events, but is constantly showing its indivisible nature by adaptation from moment to moment to varying circumstances."

The third lecture is not so clear or sound. We have difficulty in following his discussion of personality and wish heartily that he had refrained from the widespread tendency of present day scientists who, having attained a certain eminence, proceed to throw everything into the hands of God.

There is, however, a great deal of merit in this book, and especially pleasing is the pungent criticism of our careless thinking these days. "It is simply metaphysics, and bad metaphysics, long out of date, to assume that physical interpretation is perfect objective interpretation."

LIFE: *Outlines of General Biology. Volumes I and II.*

By Sir J. Arthur Thomson and Patrick Geddes. Harper and Bros., New York. $15.00. 6 x 9½; xxx + 1515; 1931.

"Outlines" are the order of the day in that class of literature which is politely, and with a fine instinct not to hurt anyone's feelings, called "non-fiction." Nearly all branches of science, philosophy and history have been "outlined." The sales of such summaries of knowledge sufficiently demonstrate that they are wanted by the general reading public. They unquestionably fill a useful purpose when they are well done.

The present treatise is by two distinguished biologists who have been collaborating since 1889. Both have always been noted for individual and original thinking. Years have tempered with wisdom these highly desirable, but in this imperfect world essentially dangerous, mental qualities. The result in the present book is a fine, thorough, and in the main sound résumé of the present status of biological knowledge and theory.

But this book is much more than a mere review. It has a definite, interesting, and valuable point of view. What this is the following quotation will make plain:

> We have illustrated, and emphasised, the indispensable rôles of Bio-Chemistry and Bio-Physics; yet we maintain as firmly that those are still but a finer Chemistry and Physics; hence not fully Physiology, as so many call them, but its preliminary and preparatory studies, and increasingly invaluable as such. Biology proper, with Physiology proper, requires those characteristic concepts of its own, which our first chapter emphasizes, and which in our last chapter we further illustrate. Thus the living being enregisters its past, both individual and racial; it exhibits purposive behaviour in its interaction with environment, and with increasing complexity. It grows and multiplies, it develops, it varies, and it evolves, as no mere physical mechanism can do, nor simply chemical process either. While there is a mechanics, chemistry, and physics of the living body, and while these are invaluably progressive alike for thought and in applications, they do not, as a matter of fact, suffice for an adequate description of Life as we know it or live it. Organic life is based on mechanism, but transcends it.
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> Accordingly we have sought to show—with, we hope, pardonable reiteration—the untenability of a Biology which denies that Mind counts. We have alternated throughout, in presenting the organism, as far as may be, now as a Body-Mind and again as a Mind-Body. Yet it will be seen that we have given to the advances of biochemistry and the physiology of hormones no less space and prominence than to the psychological side of animal behaviour, preferential mating, etc., yet for such latter discussions, and their humanly related ones, the times are surely ripe.

While intended primarily for the layman, this work will be of great value as an adjunct to the pedagogic resources of the teacher of general biology.

THE EMPEROR'S NEW CLOTHES. *Confessions of a Biologist.*

By Johan Hjort. Translated from the Norwegian by A. G. Jayne. Williams and Norgate, London. 15 shillings. 5½ x 8½; 328; 1931.

In this thought-provoking book the dis-

tinguished Norwegian marine biologist likens various modern "isms" in science and politics to the air-woven robes of the emperor in Hans Andersen's diverting tale. The two "isms" against which he directs his heaviest artillery are Einstein's concept of synchrony and Marxian socialism. Now of the two definitions of synchrony which Dr. Hjort quotes (p. 106), the first, "that the two short hands [of the clocks] indicate the same time," which he seems to favor, appears to our poor mind to be mere tautology. Synchrony is synchrony. We shall not attempt to deny it, but is a definition of any use unless it gives us a set of operations by which we can test whether two clocks at different places are synchronous or not? In short, we do not agree with him that synchrony is "a conception which exists quite independently of the question whether the synchrony of two particular occurrences can be proved in a given case."

With the biological section of the book we find ourselves more in accord. Dr. Hjort's account of his own studies of populations of fishes is most interesting. The wide variation in the annual additions to the stock of cod and herring is in marked contrast to the comparative stability of human birth rates. We agree with him also on the importance of further research on the factors which control the growth of population.

The biological viewpoint from which Dr. Hjort takes up his discussion of communism and his first-hand experience of working men make this chapter interesting reading. Our own tendency is to sympathize with him in his defense of an individualistic economic system. However, these questions are so charged with emotion that it is difficult to judge whether anyone's conclusions, *pro* or *contra*, on them are more than the rationalization of a prejudice.

REGENERATION UND TRANSPLANTATION. *II. Band: Transplantation unter Berücksichtigung der Explantation, Pflanzenpropfung und Parabiose.*

By E. Korschelt. Gebrüder Borntraeger, Berlin. 63 marks (paper); 66 marks (cloth). 6½ x 10; xx + 695; 1931.

The first volume of this compendious treatise on regeneration and transplantation was noticed in Volume III, page 131 of this REVIEW. This first part of the second volume begins the discussion of transplantation. It starts with chapters on cell fusion and explantation (tissue culture). Following this is a chapter on natural fusions and grafts in the plant world. The next eight chapters are devoted to a review of the literature on transplantation in different phyla, from coelenterates through amphibia. An interesting chapter on parabiosis leads to the two final chapters in the volume, dealing respectively with the grafting of limbs and of gills, etc.

The book covers an immense amount of widely scattered literature, thoroughly and critically. When completed it will be an invaluable reference source.

THE PROBLEM OF LIFE.

By Robert Foucher. Robert Foucher, 1219 Eye St., N. W., Washington. $1.00. 6 x 9; 24; 1931 (paper).

The author has a practical plan for somewhat indefinitely prolonging life. It includes the following elements:

1. Keeping the nostrils always coated with mentholated vaseline.
2. Gargling before each meal with H_2O_2, in a dilution of one-half.
3. Drinking a dilute solution of essence of peppermint (a few drops in a half glass of water).
4. Inhaling at least once daily a few drops of essence of peppermint in hot water.
5. Flushing the colon twice daily, night and morning, with a half glass of luke-warm water containing "some non-poisonous but efficient antiseptic," squirted from a "small hard-rubber syringe."

There you have it! In short—reach for the peppermint instead of the doc!

THE REGULATION OF SIZE AS ILLUSTRATED IN UNICELLULAR ORGANISMS.

By Edward F. Adolph. Charles C. Thomas, Springfield, Ill. $4.50. 6 x 9; iv + 233; 1931.

A useful discussion of the literature dealing with the size of unicellular organisms. There are chapters on: Body size as structure and as function; rates of growth of individuals; variability of body size; inheritance of size; the rate of multiplication; size in relation to multiplication rate; the fusion of individuals; internal characteristics and size; some external factors and size; size in relation to body surface; size of nucleus. The bibliography covers 22 pages.

A NATURALIST IN BRAZIL. *The Record of a Year's Observation of her Flora, her Fauna, and her People.*

By Konrad Guenther. Translated by Bernard Miall. Houghton Mifflin Co., Boston and New York. $5.00. 6¼ x 9½; 400; 1931.

The author of this pleasant, if somewhat rambling and discursive, collection of essays on the natural history of Brazil was invited out to Pernambuco to advise on problems of insect control. During his stay he was able to travel widely. The book was written primarily for a popular audience. It contains a wealth of interesting observations on the fauna and flora, with discussions of their meaning. The illustrations are numerous and well reproduced. There is an adequate index.

HOLIDAY HILL.

By Edith M. Patch. The Macmillan Co., New York. $2.00. 7½ x 8; 135; 1931.

This book is the third of a series of nature books for the young. The author has displayed her usual charm and skill in narrative, and Holiday Hill ought to please children quite as much as Holiday Meadow and Holiday Pond have. The science is accurate in spite of being considerably dressed up.

THE MEDIAEVAL LATIN VERSIONS OF THE ARISTOTELIAN SCIENTIFIC CORPUS, WITH SPECIAL REFERENCE TO THE BIOLOGICAL WORKS.

By S. D. Wingate. The Courier Press, London. 10s. 6d. net. 6½ x 9¾; viii + 136; 1931; paper.

An excellent and scholarly account of the manner in which the Aristotelian scientific works were restored to Western Europe at the end of the Dark Ages. All of the translators are critically considered with great thoroughness. This treatise should be welcomed by historians of scientific thought. A reading knowledge of Latin will considerably facilitate its use.

NATURE NARRATIVES. *Volume II.*

By Austin H. Clark. The Williams & Wilkins Co., Baltimore. $1.00. 4⅞ x 7¼; viii + 100; 1931.

Fifty more authoritative articles written in a popular style. The first volume was reviewed in Volume V of this Journal.

AUS DEN WERKSTÄTTEN DER LEBENSFORSCHUNG.

By Paul Weiss. Julius Springer, Berlin. 4.80 marks. 4½ x 7¼; 192; 1931.

One of a series of popular scientific books, some of which we have previously noticed here. The present volume gives a readable account of the methods of biological investigation.

HUMAN BIOLOGY

ON THE EDGE OF THE PRIMEVAL FOREST. *Experiences and Observations of a Doctor in Equatorial Africa.*

By Albert Schweitzer. Translated by C. T. Campion. The Macmillan Co., New York. $2.00. 5 x 7½; 180; 1931.

JUNGLES PREFERRED.

By Janet Miller. Houghton Mifflin Co., New York. $3.50. 5½ x 8; 321; 1931.

These two fascinating books deal with the experiences of medical missionaries in tropical West Africa. Dr. Schweitzer's was first published in 1922, and has been reprinted many times since. Dr. Miller tells of more recent experiences. In quite different ways both authors have a seduc-

tive manner of telling their stories. They both make perfectly clear, emphasize, and even dwell upon the dreadful conditions of living which a white person must suffer in tropical Africa from the heat, the humidity, the monotonous food, the insect, reptilian and various assorted *life* which cannot be escaped, the constant menace of disease, and the ever-growing lassitude. None the less both books somehow make the reader want to go out and try it! The explanation of this paradox seems, however, clear upon analysis. Both Dr. Schweitzer and Dr. Miller are, in the first place, plainly quite extraordinary persons. Dr. Schweitzer, who is one of the foremost authorities in the world on Bach and his music, carried his grand piano to his mission station and played on it every day! Dr. Miller carried on her research work on sleeping sickness through all her body and soul wracking routine duties as the mission station physician, with almost no help except from natives whom she had to train as she went along. In the second place, it is the vitality and interest of their human contacts and experiences which make their books so fascinating. By comparison the real awfulness of the physical environment seems unimportant.

Altogether we cannot too strongly recommend these two books to our readers. If all missionaries were Schweitzers or Millers the general repute of that profession would be higher than it is.

THE BIOLOGY OF CIVILIZATION.
By C. C. Walker. The Macmillan Co., New York. $3.25. 5½ x 8⅜; vii + 323; 1930.

The author of this book is an officer in the Royal Canadian Air Force, with a background of entomological work in the Malay States, Burma, the Philippines, and China before the war. In spite of some plain defects in the manner of presentation of the case, too hasty writing, and a regrettable tendency to lean heavily on second-hand historical documentation, this book is, in our opinion, a contribution of major importance to the literature of human biology. It is a realistic treatise, which attempts a broad, sweeping view of the evolution of man, considered as an organism which achieved civilization. The idea behind the book is that "history can be shown to be the record of social organization, and that such evolution conforms to definite law."

The basic biological elements, which give the key to man's social evolution, are three: food, security, and propagation. The bulk of the book is taken up with the presentation of detailed evidence to show that all human history and evolution can be explained as the consequence of man's efforts to secure these three essentials for himself, individually and collectively.

Space cannot be spared to do the book justice here. But we cannot omit the summary of the discussion of war.

> Summing up, the following laws can be considered fundamental:
>
> 1. Every engineering device which facilitates transportation over natural physical barriers, lessens the security of peoples.
>
> 2. The natural striving after security by one people, that is to say, its natural endeavours to exist, must affect the security of other peoples. Because when a people endeavours to ensure its existence, by reason of its automatic reactions to the problems connected with food-supply, security, and social stability, its endeavours will conflict with the strivings of other peoples who are also subject to the same environmental problems. Each people is only trying to exist. When a people considers that its existence is threatened by a particular environment, which may have been brought about by the strivings of other peoples in their struggle for existence, to such an extent that no adaptation to the environment will suffice, it is forced to attempt to alter that environment. But another people may consider that any alteration of that environment affects its own existence.
>
> The result is war.
>
> 3. The evidence of history shows that the abolition of war in civilization has only been accomplished by the establishment of a central control.
>
> 4. War in civilization today is a natural result of the normal processes of existence among nations sovereign in their own right and free from a common control. If the evidence of the past is final, then while a common control is lacking, and nations bear the sole responsibility for the existence of their several peoples, war will remain a persistent factor in their processes of life.

It can be seen that sentimentalists are not going to like this book. Uplifters will find little in it conducive to that dubious euphoria which is achieved by the substitution of sublimated urges to well-doing for honest rye whiskey.

MIRACULOUS HEALING. *A Personal Testimony and Biblical Study.*

By Henry W. Frost. Richard R. Smith, New York. $1.50. 5 x 7⅞; vii + 175; 1931.

This book might almost have been written by the late John Pickard Owen. Its effect is strangely similar to "The Fair Haven," though this is undoubtedly unintentional on the author's part. The book opens with a set of five cases of what the author considers clear cases of divine intervention and healing. The next chapter opens:

> If I could leave my record where it stands, as given in the preceding chapter, it would not be possible for me to come to any but radical conclusions in respect to God's readiness to heal. But it is here where I must be careful not to exaggerate, and where I must be prepared to be honest, in the strictest sense of the word, whatever this may imply. It is my purpose, therefore, to add to the experiences of healing which have been mentioned some additional ones, where, though prayer was offered and faith was exercised, like healing was not given. And I would explain that the foregoing five cases of healing were chosen from the whole number which might have been given from my experience, not only because they were amongst the most notable, but also, because I desire to add to them five other cases of an almost exactly similar character.

The five additional cases are what any practising experimenter would admit ought to provide satisfactory controls. They are matched for age, sex, profession, religious belief, etc. In the author's words:

> It will be observed that I have described in the last two chapters, ten separate cases of sickness, with the details of which I am personally familiar. Also, it will be observed that God in answer to prayer, in the first five of these, gave direct and immediate healing, and in the last five, though prayer was offered, did not give such healing, but healed otherwise or did not heal at all. In addition, it will be observed that I have chosen and arranged the ten cases in such a manner as to establish a parallelism between them, each corresponding set having reference, for the most part, to similar conditions in respect to the persons who were sick, the kind of sickness which prevailed, and the person, or persons, who prayed for healing.

In the later chapters Dr. Frost examines the question of miraculous healing in its theoretical and practical aspects. His discussion of the question is thorough and impartial. He points out that

> the spiritual law which prevailed in Old Testament times, namely, that the righteous should be well and the unrighteous should be sick (Ex. 15:26, 23:25; Deut. 7:12–15), does not pertain to New Testament times, as incidents on every hand illustrate. In the old times, the Gentiles suffered physical scourges of all sorts, while the Jews, unless they were disobedient, were kept from such. And as between Jew and Jew, the godly man usually was in better health and lived longer than the ungodly one. But no such distinctions now exist. Epidemics sweep away the unconverted and the converted alike; and frequently, the godly man is more weak and sickly than the ungodly one, while the ungodly one outlives the godly one. The finest physical specimens among men of the present time are prize fighters; and it will be commonly acknowledged, whatever may be said in their favor, that they are not noted for their spirituality.

His final conclusion is that "It is, therefore, the Christian's right, particularly if human help has failed and a physical crisis has come, to offer prayer and exercise faith, and thus to give God the opportunity to prove Himself to be the faithful Creator and the Lord of the body."

NUDISM IN MODERN LIFE. *The New Gymnosophy.*

By Maurice Parmelee. Alfred A. Knopf, New York. $3.00. 5½ x 8⅛; xiv + 303; 1931.

In this book the author of *The Science of Human Behavior, Poverty and Social Progress, Personality and Conduct, Criminology*, and other works on sociology describes the *Nackstkultur* movement and the philosophy underlying it.

> Mankind has become largely cut off from nature and life is too artificial, much to the detriment of its health and happiness. Thus man can little know and understand himself, his fellows and his natural environment, and this ignorance causes much of the stress and strain of human existence. This situation is strikingly exemplified in the concealment of the body, which hampers the rearing of the young, gives rise to unhealthy mental complexes, and creates abnormal relations between the sexes. . . .
>
> The new gymnosophy endeavours. . . . to regain what mankind has lost through civilisation, without rejecting anything of human, social and cultural value. Nudity aids materially in bringing mankind closer to nature, in promoting more genuine and sincere relations between the sexes, and in rearing the young. It is symbolical of a life healthier and saner than our present hectic existence. Such a life would be possible in a more humane and democratic society, whose principal features I have attempted to outline.
>
> The illustrations depict better than words can describe the natural and normal life, and the beautiful and healthful methods and activities of a gymnosophic society.

No doubt artificial modesty is one of the more dubious of our cultural acquisitions, but, as Havelock Ellis points out in his introduction, one can attain a large part of the aims set forth in this book by the practice of nakedness privately and among personal friends without anything so ambitious as an organized "movement." Moreover the idea of a nude society excites the risibilities of one accustomed to a clothed society. To such laughter the gymnosophist would no doubt reply in the words of Plato: "But then, we must not fear the jests of the wits which will be directed against this sort of innovation. . . . . And as for the man who laughs at naked women exercising their bodies from the best of motives, in his laughter he is plucking a fruit of unripe wisdom, and he himself is ignorant of what he is laughing at, or what he is about;—for that is, and ever will be, the best of sayings, *That the useful is the noble and the hurtful is the base.*"

A Review of the Effects of Alcohol on Man.

Victor Gollancz, London. 8s. 6d. net. 5½ x 8½; 300; 1931.

A group of six persons appointed two committees to study the alcohol problem in Great Britain—one to report on the social and economic aspects, the other, on the medical or scientific. This book, the report of the medical committee, is composed of the following chapters: The physiological actions of alcohol, by Howard Florey; Alcohol as a poison, by G. Roche Lynch and W. H. Brown; Alcohol and the body's resistance to disease, by F. S. Langmead and T. C. Hunt; Mental effects of alcohol, by Bedford Pierce; Alcohol and mental disorder, by Isabel G. Wilson; The changes in the body caused by alcohol, by W. D. Newcomb and T. C. Hunt; Alcohol in the practice of medicine, by F. S. Langmead and T. C. Hunt; and The racial effects of alcohol, by F. A. E. Crew and Isabel G. Wilson.

The authors have attempted to give as impartial a review as possible of the scientific work done on this subject. They advance no clear and positive statement which might be used for legislative purposes. The most we can gather from the reading of this book is that the results here discussed tend to relieve the moderate use of alcohol from the imputations generally ascribed to it, and at the same time, to emphasize the dangers of its excessive use.

Brown America. *The Story of a New Race. By Edwin R. Embree. The Viking Press, New York.* $2.50. 5½ x 8⅛; vi + 311; 1931.

The author of this book is president of the Julius Rosenwald Foundation, which devotes a large part of its funds to the betterment of the colored race. He emphasizes the fact that there now exists very little of the original pure African stock in the United States. For roughly two hundred and fifty years after the first slaves were brought into the country a new race was in the making. The interbreeding of the blacks with Indians and whites up to the emancipation era, and since that time the breeding of these hybrid peoples among themselves, has welded them into a surprisingly uniform type, which the author calls the "brown race." The physical changes which the original African stock has undergone have been accompanied by corresponding changes in language, habits, and religion. These changes, together with the present status of Brown Americans in our economic and social life, their present educational advantages and more particularly plans for their future enlightenment form the basis of this book. Probably not all will agree as to the wisdom of forcing by hot house methods the education of a group of people whose constitutional and social upheaval has been so recent and profound.

The Negroes of Africa. *History and Culture.*

By Maurice Delafosse. Translated from the French by F. Fligelman. The Associated Publishers, Washington, D. C. $3.15. 5⅜ x 7⅞; xxxiii + 313; 1931.

The two books, *The Negroes of Africa*, and *African Negro Civilizations*, combined in this

translation were originally written for the purpose of interesting the French in their colonies in Africa and the populations which inhabited them. The author, who at the time of his death in 1926 had spent seventeen years in an administrative capacity in West and South Africa, had unusual opportunites to study the languages of the negro races, their ethnology, sociology and history. In his writings he sought to establish "a sort of synthesis of what is common to all African Negro civilizations considered by themselves and in their real character, aside from the alterations that have been brought about, in some regions, either by Mussulman or European influence. . . . with the aim of contributing to a more exact knowledge of their collective mentality and their institutions." The student rather than the general reader will find this book of interest. It is well documented and has a name and general index.

NEGRO YEAR BOOK. *An Annual Encyclopedia of the Negro, 1931–32.*
Edited by Monroe N. Work. Negro Year Book Publishing Co., Tuskegee Institute, Ala. $2.00. 6 x 9⅛; xv + 544; 1931.

This standard reference book, now in its eighth edition, is divided into the following parts: the Negro in the United States; the Negro in Latin America; the Negro in Europe; the Negro in Africa; the Negro in poetry and fine arts; literature on the Negro; directory of newspapers, agencies and organizations; and statistics showing racial distribution of mankind. Much of it is a record of praiseworthy achievement against great difficulties and may remind us that the day is getting nearer when, in the words of one Negro editor, "the Negro shall add his quarto to American civilization."

A GUIDE TO HISTORICAL LITERATURE.
Edited by William H. Allison, Sidney B. Fay, Augustus H. Shearer, and Henry R. Shipman. The Macmillan Co., New York. $10.50. 5¾ x 8½; xxviii + 1222; 1931.

An annotated bibliography covering the field of history of all ages and of all countries. It has been meticulously compiled by a Committee on Bibliography of the American Historical Association, with the coöperation of numerous historians and other scholars. While planned especially for the librarian and general reader, the needs of the special student of history have been kept in mind. In the selection and arrangement of material those nations of greatest cultural importance, and especially their periods of greatest development and influence, have received predominant space. Besides political history, the work includes writings on diplomatic, military, economic, social, religious and cultural history. Selected lists of biographies are given, also source books, periodicals and archive publications. Students of science will find this volume a highly useful guide in orienting the history of Science with general history.

MÄNNER GEGEN TOD UND TEUFEL.
By Rudolf Thiel. Paul Neff Verlag, Berlin. 7.50 marks. 5½ x 8¾; 416; 1931 (paper).

The author presents in this book twenty-one great pathfinders of medicine, men who fought "against death and the devil," disease and superstition: Vesalius, Paracelsus, Weyer, Harvey, Haller, Frank, Mesmer, Jenner, Auenbrugger, Laënnec, Hahnemann, Priesnitz, Müller, Dieffenbach, Schönlein, Helmholtz, Graefe, Semmelweiss, Virchow, Pettenkofer (with incidental reference to Koch), Bergmann. The dramatic manner in which he portrays these eminent physicians as living personalities filled with a fanatic zeal for knowledge and an intense love for suffering humanity makes the book most fascinating and stimulating reading. At the same time we become acquainted with their works, their epochal discoveries and inventions, in all essential features. Seventeen illustrations still further enhance the value of this book, a brilliant addition to the popular literature on the history of medicine.

THE JAPANESE POPULATION PROBLEM. *The Coming Crisis.*

By W. R. Crocker. George Allen and Unwin, London. 10s. 6d. net. 5½ x 8½; 240; 1931.

A careful study of the present and prospective situation of Japan's population. The author expects an increase of fifteen to twenty millions in the Japanese population in the next twenty to thirty years. The present area of Japan cannot feed such a number; rice is being imported now. It is doubtful whether Japanese industry can support the prospective increased population. The author believes that the most promising avenue of relief is emigration, especially to Brazil; but even this he thinks will be of greatest value as a psychological safety-valve. The temper of the book is even; there is no attempt to stir the reader to action. There is a bibliography and index.

ANCIENT AMERICANS. *The Archæological Story of Two Continents.*

By Emily C. Davis. Henry Holt and Co., New York. $3.50. 5¼ x 8; xii + 311; 1931.

Miss Davis' experience on the staff of Science Service, where she reports recent archæological discoveries, has well fitted her to write this book for the general reader. She gives an extensive survey of present day knowledge concerning the early dwellers of the two Americas, reconstructing so far as possible the lives of these people whose widely scattered remains indicate highly diversified ways of living. For each important group (the Mound Builders, the Aztecs, Basket Makers, Incas, etc.) she discusses the outstanding features of their civilization. Of much interest are those sections dealing with the archæologist's methods of exploration and technique in interpreting his findings. The book is well illustrated and contains a bibliography and an index. Teachers of high school students would do well to include it in their lists of suggested reading, since there is much within its covers that will be stimulating to the youthful mind.

THE JONES COVE SHELL-HEAP AT WEST GOULDSBORO, MAINE. *Lafayette National Park Museum Bulletin I.*

By Walter B. Smith. Lafayette National Park Museum, Bar Harbor, Me. 35 cents. 6 x 9; 28; 1929 (paper).

THE LOST RED PAINT PEOPLE OF MAINE. *A Few Things We Think We Know About Them and More That We Know We Don't. Lafayette National Park Museum Bulletin II.*

By Walter B. Smith. Lafayette National Park Museum, Bar Harbor, Me. 35 cents. 6 x 9; 43; 1930 (paper).

These two bulletins, the first to be issued by the newly founded Lafayette National Park Museum of Stone Age Objects, describe in detail (1) the contents of the shell-heap at Jones Cove which has recently been opened; and (2) traces of a people that left many relics wholly unlike those of the stone-age culture. There are indications that the red paint people were of great antiquity. The author suggests the possibility that they, together with their village sites and most of their burial grounds, were wiped out of existence by the subsidence of the fringe of the Maine coast below sea level from two to four thousand years ago.

WOMEN FOR SALE.

By Chile Acuna. William Godwin, New York. $2.00. 5 x 7½; xii + 201; 1931.

This narrative of one of the chief witnesses before the Seabury Committee reminds us that there are chains of causation in society as complex and wonderful as any of those in nature so dear to the heart of the natural theologian. The godly ordain that prostitution shall be suppressed by law. The members of the police detailed to this pious duty soon find that their bank accounts increase more rapidly if, instead of arresting the prostitutes, they accept protection money from them. But efficiency demands that someone shall be arrested. Therefore innocent women do time for offenses that they have never committed. All of which fills one with renewed admiration for the wisdom and goodness of that Power by which the universe is ordered.

RACE AS A POLITICAL FACTOR. *Conway Memorial Lecture, Delivered at Conway Hall, Red Lion Square, W. C. 1, on April 15, 1931.*

By J. W. Gregory. Watts and Co., London. 2 shillings net. 4 x 6½; 72; 1931.

This lecture is a summary of what we know ethnologically and anthropologically concerning races. The author concludes that segregation of the three primary races is desirable, from the observation that cross breeding in plants and animals and humans of very dissimilar race characteristics tends to produce inferior progeny. The suggestions offered for establishing segregation, particularly negro-white segregation in the United States, do not seem highly practical or very easy to put into effect.

NEW CENSUS ATLAS OF THE WORLD. *Complete 1930 Census.*

Edited by Frederick K. Branom. The Reilly and Lee Co., Chicago. $3.50. 10¼ x 14½; iv + 256; 1931.

The atlas includes not only all the usual up-to-date maps, but a wide variety of useful information, such as census counts of all the important localities, informative geographical texts, interesting pictures of scenes and people, a brief account of the history of geography and exploration, the solar system, and a meager geological description of the earth as a whole, the continents, islands, oceans, etc. Proportionally most space is devoted to the United States of America. There is a geographical glossary.

FAMILY ORIGINS *and Other Studies.*

By J. Horace Round. Edited with a Memoir and Bibliography by William Page. Richard R. Smith, New York. $7.50. 5½ x 8¾; lxxiv + 303; 1930.

In one of these papers Round confesses, with his usual candor, that the service which genealogy can render to the general historian can easily be overrated. The same might be said of its service to the student of heredity. However this may be, it is certain that the type of genealogical work which can be useful to the geneticist is the careful research on historical principles which Round himself practised rather than that the chief aim of which is to gratify the vanity of the compiler.

LOVE-CHILDREN. *A Book of Illustrious Illegitimates.*

By Miriam A. de Ford. The Dial Press, New York. $3.50. 5½ x 8; 302; 1931.

As illustrious illegitimates, the author has chosen: as king, William the Conqueror; as statesman, Alexander Hamilton; as scholar, Erasmus; as painter, Leonardo da Vinci; as scientist, D'Alembert; as composer, Borodin; as littérateur, Strindberg and Dumas, fils. The life and work of these are studied with reference to the sociological implications of their birth. In a prologue the author deals briefly with about fifty more illegitimates who have become famous, and discusses the general subject of illegitimacy, especially the attitude of society and the law in various countries during different periods of history. An epilogue gives some scientific and statistical data.

THE NEW ROAD TO CIVILIZATION. *Volumes I and II.*

By H. G. Baker. Economy Publishers, Seattle. $3.20 for two volumes. 4 x 6⅞; Vol. I, 150; Vol. II, 170; 1931.

According to the Professor of Biology at Dakota Wesleyan University the new road to civilization is eugenics, as to the results of which he is sufficiently sanguine.

> 9 months after eugenics is put into practice, the world begins to rise to higher levels! Five years after eugenics begins to operate in any community, the school children begin to show more intelligence and require less effort on the part of the teacher. Little faces are brighter, little souls are purer, little hearts are gayer, little bodies are stronger and freer from suffering and disease!

SIAM: *Nature and Industry 1930.*

Ministry of Commerce and Communications, Bangkok. Baht 3.00 + 85 satang postage. 6½ x 9½; v + 324; 1930.

This volume is partly based on a series of

pamphlets which first appeared in 1926. The purpose of the book is to issue in concise form all the important data concerning the natural features, the industry and commerce of Siam, so that the interested individual may readily inform himself concerning Siam's position as a producing country. Each of the twenty-two chapters has been compiled by a specialist. The volume is excellently illustrated with maps, charts and photographs. There is an index.

DIE RASSISCHEN VERHÄLTNISSE IN NORDEUROPA *nach dem gegenwärtigen Stand der Forschung.*

By Walter Scheidt. E. Schweizerbart'sche Verlagsbuchhandlung (Erwin Nägele), Stuttgart. 43 marks (paper); 45 marks (cloth). 6½ x 10; 197 + 12 plates + 31 maps; 1930.

This work deals with the *Rassenkunde* of Great Britain, Iceland, Norway, Sweden, and Denmark. The data were collected according to anthropometric and statistical methods outlined in the author's "Allgemeine Rassenkunde," previously reviewed in these columns. His thorough and critical treatment represents a study of some 85,000 persons and 4,727 skulls, and should be of interest not only to anthropologists, but to anatomists and historians as well. Its usefulness is augmented by indices and an extensive bibliography.

A BIBLIOGRAPHY OF SOCIAL SURVEYS. *Reports of Fact-Finding Studies Made as a Basis for Social Action: Arranged by Subjects and Localities. Reports to January 1, 1928.*

By Allen Eaton, in Collaboration with Shelby M. Harrison. Russell Sage Foundation, New York. $3.50. 6 x 9; xlviii + 467; 1930.

A detailed bibliography of 2,775 surveys and social studies, both specialized and general. Information concerning the subject, author, date, number of pages, price, and where obtainable, are all given. It should be a valuable reference source for those interested in social survey work. There is an introduction explaining at some length the history and interpretation of the social survey.

THE BALANCE OF BIRTHS AND DEATHS. *Volume II. Eastern and Southern Europe.*

By Robert R. Kuczynski. The Brookings Institution, Washington. $2.00. 5¼ x 8; xii + 170; 1931.

The second volume of the author's studies on this subject. It deals with the countries of Eastern and Southern Europe. The same tendency to a decline in net reproduction is apparent in the case of most of the countries here studied, though it has not gone so far as in the Western European countries. Russia shows the greatest net reproduction rate, and one which is practically unchanged from thirty years ago.

THE FOUNDATIONS OF MEDICAL HISTORY.

By Sir D'Arcy Power. The Williams & Wilkins Co., Baltimore. $3.00. 4¾ x 7¼; x + 182; 1931.

Six lectures on cultural and humanizing aspects of medical science presented in an authoritative and charming manner. The topics dealt with include: The story of the oldest British hospital (St. Bartholomew's, with which the author has been connected for the last 52 years); Dining with our ancestors; Medical biography; Medical Iconography; Medical bibliography; and "Aristotle's Masterpiece." The introduction is by Dr. William H. Welch.

CHILD WELFARE IN SELECTED COUNTIES OF WASHINGTON. *United States Department of Labor, Children's Bureau Publication No. 206.*

U. S. Government Printing Office, Washington. 20 cents. 6 x 9; v + 111; 1931 (paper).

A dull, detailed description of means taken for the care of dependent and delinquent children in six counties of the State of Washington. Its hundred odd pages suggest more than anything else that by

curtailing the sort of activity that produced them Congress has at hand a real, if minor, way of helping to balance the budget, without causing serious or widespread pain to the taxpayers of the nation.

THE CARE OF THE AGED. *Proceedings of the Deutsch Foundation Conference, 1930. Social Service Monographs, Number Fourteen.*

Edited by I. M. Rubinow. University of Chicago Press, Chicago. $2.00. 6 x 9; xiii + 144; 1931.

Most of these papers are concerned in the main with the social and economic aspects of the care of the aged. Two of them, however, deal specifically with the medical side of the question. There are appendices on the care of the aged in Illinois and on state laws on old age security, and an index.

LOOKING INWARDS.

By Veronica and Paul King. Heath Cranton, London. 7s. 6d. net, postage 6d. extra. 5½ x 8¾; 209; 1931.

An entertaining book, which sets forth in sufficient detail the sad story of how idiotically the English manage their affairs, social, political, economic, and so on. The authors apparently have a *libido* for this sort of thing. They have tacked our own Uncle Sam's hide to the barn-door in an earlier *opus*. But it must be said that they do it well. Oddly enough they are both smart and sincere. There is an index.

THROUGH THE LANDS OF NYANZA. *Central Africa.*

By Wm. J. W. Roome. Marshall, Morgan and Scott, London. 5 shillings. 5 x 7⅝; 208 + 1 map; no date.

This book is devoted largely to an account of the establishment of mission schools and hospitals in Uganda and the spread of Christianity among the natives. It was written evidently for propaganda. It has little biological interest.

MARRIAGE AND FAMILY LIFE AMONG STRANGE PEOPLES. *Studies of Actual Savage Life.*

By Louise M. Spaeth. Thomas S. Rockwell Co., Chicago. $1.25 net. 5½ x 8; 111; 1931.

A brief account of marriage and tribal customs of primitive peoples suitable for those who have neither time nor energy to read the classics on the subject. There is nothing new; only a summary of what has already been better said by the authorities.

THE BLACK DEATH AND MEN OF LEARNING.

By Anna M. Campbell. Columbia University Press, New York. $3.00. 5½ x 8; xii + 210; 1931.

A study of the relations of the Black Death of 1347–1350 to the intellectual life of the time. Particular attention is paid to the tractates written by contemporary physicians on the disease, and to the effects of the plague on the universities. The book is a real contribution to the history of science and of medicine.

L'ŒUVRE DU SERVICE DE SANTÉ MILITAIRE EN ALGÉRIE, 1830–1930. *Ministère de la Guerre, Direction du Service de Santé.*

Charles-Lavauzelle et Cie, Paris. 15 francs. 5½ x 8¾; 363; 1931 (paper).

A history, prepared under the direction of the Health Service of the Ministry of War, of the progress in the development of surgery, medicine, bacteriology, hygiene and sanitation, and epidemiology in Algeria during the century ending 1930. Short sketches of the life and work of the principal men connected with this work are included.

HANDS OF DESTINY. *Your Life's Fate in the Lines of Your Palm.*

By Josef Ranald. Greenberg: Publisher, New York. $2.00. 5¼ x 8; 181; 1931.

A treatise on palmistry.

ZOÖLOGY

FLOWERS AND INSECTS. *Lists of Visitors of Four Hundred and Fifty-Three Flowers.* *By Charles Robertson. The Science Press Printing Co., Lancaster, Pa.* $3.00 net. 6½ x 9½; 221; 1928.

For more than forty years Professor Robertson has been recording his observations on the species of insects visiting the different species of plants growing near Carlinville, Illinois. The present book records 15,172 visits. One of the great difficulties of the work was the identification of the insects captured, and the author's remarks on systematists are not without piquancy.

> Generally, determinations of bees are unreliable except when both sexes of all the species are known. When only one sex is known the other is probably mixed with that of another species. Formerly in *Agapostemon* four species had been distinguished in the females but the males were referred to another species, *A. tricolor*. There was little chance of separating another real species and this condition had the advantage of indicating five species instead of four. Sometimes the species are separated only in the males and the females ignored. Determinations of the females are twice as important as those of the males since they make twice as many flower visits. . . .
>
> From my experience I think that a specialist is competent to make determinations, or compare types, only at the time when he is making a careful study of a particular group. In 32 cases I received two different specific names for the same insect.
>
> Your species monger describes the obvious and easy cases, the males and females as distinct species, and passes on the difficulties to some one else. North America is a large region and entomology, or even an order of insects, is a large subject. A specialist's reliability bears an inverse relation to the size of the region and the extent of the subject in which he affects authority. Students of bees describe species from all over North America, and even farther, but any one of them, and all of them together, can not identify both sexes of the bees of any locality.
>
> All of the observations given here were made for the purpose of ascertaining the different kinds of insect visitors. Collectors' notes are worthless as data of anthecology because the collector is looking out for particular kinds of insects and not for all of the kinds which occur on the flowers. They are worthless as data of insect habits because the collector is certain to select the flowers which are most favorable for observation and which are most attractive to the kinds of insects he is after. Collectors' notes almost invariably fail to indicate what the insects are doing on the flowers and whether they affect pollination or not.

SNAKES OF THE WORLD. *By Raymond L. Ditmars. The Macmillan Co., New York.* $6.00. 7½ x 10½; xi + 207 + 84 plates; 1931.

The wide experience of the writer of this book, as curator of reptiles at the New York Zoölogical Gardens for over thirty years and as extensive traveler and collector in temperate and tropical countries throughout the world, enables him to write authoritatively concerning these interesting if generally fearsome animals. Guided very largely in the preparation of the volume by queries about snakes, he has produced a work of great practical interest as well as of scientific value. Unnecessary technicalities have been avoided. Following the introductory chapters and the section on classification, in which the author discusses relationship from new angles, the subject is generally divided into sections on the giant serpents, harmless snakes and poisonous snakes. The poisonous snakes for each country are grouped together so that sportsman and traveler will have necessary information readily available. Much hitherto unpublished material on habits is presented. The large number of excellent photographic plates form an important part of the book. It is well indexed.

THE PLANT LICE, OR APHIIDAE, OF ILLINOIS. *State of Illinois Department of Registration and Education, Division of the Natural History Survey, Vol. XIX, Article III.* *By Frederick C. Hottes and Theodore H. Frison. Natural History Survey, Urbana.* Free. 6¾ x 10¼; 326; 1931 (paper).

This extensive survey of plant lice in the State of Illinois covered a period of three years (1928–1930). It is a purely faunistic study and deals with the number of kinds of plant lice found, their characteristics, distribution, host relationships, seasonal adjustments, importance as potential enemies and a general concept of their life histories. The keys, arranged to make identification as easy and simple as possible, have been prepared almost entirely on the basis of the alate viviparous females, the forms usually taken in the field. About 100 species have been added to those already reported for the State. Of these 36 are new to science. The bulletin includes a number of photo-

graphs, many drawings, a host index, bibliography, and general index.

WILDERNESS TRAILS IN THREE CONTINENTS. *An Account of Travel, Big Game Hunting and Exploration in India, Burma, China, East Africa and Labrador.*

By Lionel A. D. Leslie. Heath Cranton, London. 10s. 6d. net (postage 6d. extra). 5⅜ x 8½; xvi + 223; 1931.

This book takes the reader into little known places in different parts of the world. The author, while performing the duties of a British soldier, traveled and hunted much in Northern India, Burma, Thibet, Western China, and the African bush, always in regions occupied mainly by primitive peoples and wild animals. In Labrador, as a member of the Royal Geographical Expedition, he penetrated far into the interior of the country, exploring by canoe the Renamu River to its source. During the nine years of travel he had many thrilling and amusing experiences. Possessing a quiet humor and writing simply and unaffectedly, his book is always entertaining whether he writes of his experiences as a big game hunter, of ancient villages and temples, tribal customs and religious ceremonies or scenery. Many excellent photographic plates and a map of the Indian-Thibet-Burma region give added interest to the book. Winston Churchill contributes a brief foreword. There is an index.

THE ARACHNID ORDER CHELONETHIDA. *Stanford University Publications, University Series, Biological Sciences, Volume VII, Number 1.*

By Joseph C. Chamberlin. Stanford University Press, Stanford University, Calif. $1.00 (paper); $1.50 (cloth). 7 x 10; 284; 1931.

In this study of the pseudo-scorpions, to which the author has given many years of work, the systematics of the order have been completely reorganized. The paper is in two sections. The first part deals with the morphology of the exoskeletal structures with a full discussion of their range of form within the order, taking the family as the unit of comparison. The second section is devoted to the classification of the order. This, in the main, is general, the work not being carried past the families, of which eighteen are listed. Species have been dealt with only as illustrative material for the higher groups.

This treatise will be of basic value to future workers in this group of arachnids, both in the field of comparative morphology and of classification. The work is extensively illustrated, mostly with the author's own drawings. There is a bibliography of 311 titles and an index.

THE FAUNA OF BRITISH INDIA, INCLUDING CEYLON AND BURMA. *Reptilia and Amphibia. Vol. I. Loricata, Testudines.*

By Malcolm A. Smith. Taylor and Francis, London. 15 shillings. 6 x 9; xxviii + 185 + 2 plates; 1931.

This treatise on Loricata and Testudines is the first of four volumes to be devoted to the reptiles and amphibia of British India. The area covered is practically the same as that covered by Gunther in his *Reptiles of British India*, published in 1864. This means present-day India, with the addition of the Indo-Chinese sub-region; an area of some 2,350,000 square miles, possessing every variety of elevation and climate. In the descriptive notes of the different forms the author devotes much attention to habit and distribution. The work includes illustrations, a glossary of technical terms, a bibliography, and a systematic and alphabetical index. An interesting feature of the introduction is the series of short biographical sketches of the best known Indian and Indo-Chinese herpetologists.

THE BIOLOGY OF THE AMPHIBIA.

By G. Kingsley Noble. McGraw-Hill Book Co., New York. $5.00. 5¾ x 9; xiii + 577; 1931.

The author of this volume, which is one of the outstanding textbooks to be issued in recent years, is curator of herpetology and experimental biology at the American Museum of Natural History. He presents a thoroughly comprehensive treatise on

the biology of the amphibia and the relationships of the different forms. Although written for the student and investigator, the clear and interesting style of writing, the omission of technicalities wherever possible and the inclusion of a great deal of natural history, make the book far more interesting than the average textbook. The sections on physiology are necessarily abridged, but the carefully selected reference list appended to these, as well as to all other sections of the book, assists those who wish for more detailed reading. There are numerous illustrations and a detailed index.

CORAL REEFS AND ATOLLS. *Being a Course of Lectures Delivered at the Lowell Institute at Boston February 1930.*

By J. Stanley Gardiner. The Macmillan Co., New York. $4.25. xiii + 181; 1931.

The author of these lectures spent five years on coral reefs and atolls making biological observations. In this interesting and clearly-written book he presents much information about fringing, barrier and atoll reefs; island formations; the natural history of corals; other plant and animal reef builders; the other organisms of reefs; the distribution of coral reefs; atolls and their lagoons; the foundations of atolls. He reviews critically, in the last lecture, the theories which have been advanced on the formation of coral reefs and concludes that "we are forced to admit that all our theories and considerations are mere camouflage for our lack of knowledge."

SYSTEMATIC REVISION AND NATURAL HISTORY OF THE AMERICAN SHRIKES (LANIUS). *University of California Publications in Zoölogy, Vol. 38, No. 2.*

By Alden H. Miller. University of California Press, Berkeley. $3.00. 7 x 10¾; 242; 1931 (paper).

In this study on speciation the American shrike was selected because of the "relative incipiency of the differentiation within the group." The author used 2,083 shrike skins in making the systematic revision of the group. He includes studies on shrike behavior, distribution of geographic races and natural history, and reports two heretofore unknown races of *Lanius ludovicianus*. In the characterization of the various geographic forms a statistical study of the external dimensions of age and sex groups was made. The work includes statistical tables and graphs, distribution charts and photographs. There is a lengthy bibliography but no index.

COMMON PESTS. *How to Control Some of the Pests that Affect Man's Health, Happiness and Welfare.*

By Rennie W. Doane. Charles C. Thomas, Springfield, Ill. $4.00. 5¼ x 8; viii + 397; 1931.

A handy reference book which will be found especially useful by those living in rural communities. The author deals in non-technical language with the pests of man and domestic animals, as well as those of the orchard, garden, field and household. He gives the necessary information concerning the habits and life histories of these animals and the generally accepted methods of their extermination. The many excellent illustrations are well chosen and add greatly to the usefulness of the book. There is an index.

A DICTIONARY OF GREEK AND LATIN COMBINING FORMS USED IN ZOÖLOGICAL NAMES.

By Edmund C. Jaeger. Charles C. Thomas, Springfield, Ill. $1.50 (1 to 5 copies); $1.00 if remittance accompanies order; $1.00 in quantity lots for classes. 4 x 7; iv + 157; 1931.

In the old days, when a knowledge of Greek and Latin was a part of the equipment of every educated man, zoölogical terminology meant something to the beginner. Nowadays it too often seems to him merely a collection of outlandish words, the meanings of which he has to learn by rote. To remedy this sad state of mind is the object of this excellent little

book, which we commend to all teachers of zoölogy. Over 1,200 commonly used Greek and Latin forms that enter into the make-up of zoölogical terms and classificatory names, are catalogued, concisely defined, and illustrated by examples.

SOCIAL BEHAVIOUR IN INSECTS. *The Dial Press Monographs on Biological Subjects.*

By A. D. Imms. The Dial Press, New York. $1.50. 4 x 6½; ix + 117; 1931.

A brief discussion of the structure and habits of the social insects, their evolution and the basis upon which their behavior depends, written mainly for the investigator who, working in other fields of biology, has little time to devote to this highly specialized field. Students, teachers, and the general reader will also find this little book of interest. It contains a number of illustrations, an appendix on literature, and an index.

THE STANDARD NATURAL HISTORY *from Amoeba to Man.*

Contributors: G. J. Arrow, M. Burton, W. T. Calman, J. G. Dollman, F. W. Edwards, C. C. A. Monro, J. R. Norman, H. W. Parker, W. P. Pycraft, N. D. Riley, G. C. Robson, Theodore H. Savory. Edited by W. P. Pycraft. Frederick Warne and Co., New York. $6.00. 6¼ x 8¾; xiv + 942; 1931.

An excellent one-volume, semi-popular survey of the animal kingdom, written by some dozen members of the staff of the British Museum (Natural History). It is as entertaining as it is reliable. There are over 900 illustrations, mostly original and nearly all good. In the text much space is given to habits and ecology, which is sound tactics. We know of no comparable book so good as this is for its purpose. It is the long-desired really satisfactory one-volume natural history.

THE INSECT MENACE.

By L. O. Howard. The Century Co., New York. $3.50. 5½ x 8¼; xv + 347; 1931.

An interesting and well illustrated popular account of the damage done by insects, and of the efforts which have been made to control them. Whether insects are the immediate danger to humanity that is here implied is another question; but the book should furnish both instruction and entertainment.

TO THE ENDS OF THE WORLD AND BACK. *Scouting for a Great Museum.*

By J. Walker McSpadden. Thomas Y. Crowell Co., New York. $3.00. 5½ x 8⅛; xv + 362; 1931.

The author interviewed fifteen field men connected with the American Museum of Natural History, and in this book has set down, for the most part in their own words, their adventures while collecting specimens in various parts of the world. The stories make interesting and, at times, exciting, reading.

BUTTERFLY AND MOTH BOOK. *Personal Studies and Observations of the More Familiar Species.*

By Ellen Robertson-Miller. Charles Scribner's Sons, New York. $2.50. 5 x 7¼; xxii + 285; 1931.

A book, written in the popular style, on the life histories of the more familiar species of moths and butterflies. It is well illustrated by drawings and photographs.

ANIMAL ECOLOGY. *With Especial Reference to Insects.*

By Royal N. Chapman. McGraw-Hill Book Co., New York. $4.00. 5¾ x 9; x + 464; 1931.

This book is a development of the author's mimeographed text, noticed here in Vol. 3, No. 3. It retains the merits of the earlier volume, specifically the emphasis on precise quantitative work, and the valuable literature lists. An appendix by Volterra makes this author's mathematical theory of variations in numbers of animal species readily available to the English-speaking reader. An excellent and useful piece of work.

THE BIRDS OF OKLAHOMA. *Revised Edition. Publications of the University of Oklahoma, Vol. III, Biological Survey, No. 1.*
By Margaret M. Nice. University of Oklahoma Press, Norman. $2.25. 6 x 9⅛; 224; 1931 (paper).
This survey aims to present a history of Oklahoma birds from 1820 to the present time. Three hundred eighty-five species and subspecies (10 of which are now extinct in the State) are described. A valuable book for the student and lover of bird-lore.

THE RIDDLE OF MIGRATION.
By William Rowan. The Williams & Wilkins Co., Baltimore. $2.00. 5¼ x 7¾; xiv + 151; 1931.
An interesting discussion of the problem of bird migration and of the author's experimental investigation of the problem. He believes that the variations in length of day, acting through a hormone produced by the interstitial tissue of the gonads, are the primary stimuli leading to migration.

SALMON HATCHING AND SALMON MIGRATIONS. *Being the Buckland Lectures for 1930.*
By W. L. Calderwood. Longmans, Green and Co., New York. $1.80. 4¾ x 7¼; 95; 1931.
An interesting little book on the habits of salmon, based on prolonged observations made by the author. The book has great practical value for those interested in preserving and increasing the supply of our salmon. It is open to speculation whether or not our present artificial methods of hatching are any improvement upon nature's.

A TEXT-BOOK OF THE DISEASES OF THE SMALL DOMESTIC ANIMALS.
By Oscar V. Brumley. Lea and Febiger, Philadelphia. $4.75 net. 5¼ x 7¾; 611; 1931.
This excellent text on the diseases of the dog, the cat and the rabbit, will be useful to those who have the care of laboratory animals.

BREEDING AND CARE OF THE ALBINO RAT FOR RESEARCH PURPOSES.
By Milton J. Greenman and F. Louise Duhring. Wistar Institute of Anatomy and Biology, Philadelphia. $3.00. 6⅝ x 9⅝; 121; 1931.
A second edition of a book of great value to all workers with rats. No great changes have been made, though numerous minor modifications and amplifications of the original text will be found.

THREE SCOUT NATURALISTS IN THE NATIONAL PARKS.
By Donald G. Kelley, Jack W. Edgemond, and W. Drew Chick. Brewer, Warren and Putnam, New York. $1.75. 5¼ x 7⅞; 237; 1931.
This entertaining narrative describes the adventures of the authors during a summer as field assistants to the Senior Naturalist and Forester of the National Park Service. We commend it, not only to all Boy Scouts, but to all who enjoy lively books of travel.

STRANGE ANIMALS I HAVE KNOWN.
By Raymond L. Ditmars. Brewer, Warren and Putnam, New York. $3.50. 5½ x 7⅞; 375; 1931.
An interesting and amusing book dealing with experiences in collecting and keeping wild animals. The author is curator of mammals and reptiles in the New York Zoo, and has a wide acquaintance with the manners and customs of animals.

BOTANY

BLUTENBILDUNG UND SPROSSGESTALTUNG (*Anthokladien und Infloreszenzen*). *Zweiter Ergänzungsband zur Organographie der Pflanzen.*
By K. Goebel. Gustav Fischer, Jena. 16 marks (paper); 18 marks (cloth). 6¾ x 10⅛; vii + 242; 1931.
This supplementary volume to the third edition of Goebel's classic of botanical literature, the *Organography of Plants*, deals

with the anatomy and morphogenesis of inflorescences. It is unnecessary to say that, like all of Goebel's work, this maintains the highest standards of sound biological methodology. While in the main the book is descriptive morphology, rather than experimental, it is description informed and illumined with philosophical insight, and interpreted dynamically. Thus differences in inflorescence formation within natural taxonomic groups are shown to be due to differences in distribution of growth; contraction or decreases and multiplication of flowers; fusion or failure to separate of branch elements of inflorescences; or, most important of all, changes in rhythm and symmetry. The book is copiously and effectively illustrated, and has an adequate index.

THE GENERA OF FUNGI.

By Frederic E. Clements and Cornelius L. Shear. The H. W. Wilson Co., New York. $8.50. $6\frac{3}{4}$ x $9\frac{3}{4}$; iv + 496 + 58 plates; 1931.

In this second edition the number of generic names included has been increased from 2,909 to more than 5,000, the authors having attempted to account for all genera proposed since the first edition. The book contains a key to the genera with references to descriptions, a list of type species and synonyms, a bibliography of nineteen pages, glossary, index, and about 1,800 figures, illustrating the type or other representative species of approximately 700 genera. It will be invaluable to the mycologist.

THE PRINCIPLES OF PLANT BIOCHEMISTRY. *Part I.*

By Muriel W. Onslow. The Macmillan Co., New York. $4.75. 6 x $9\frac{1}{4}$; 326; 1931.

In this book, which deals only with the higher plants, the author of *Practical Plant Biochemistry*, reviewed in Vol. V, considers the biochemistry of the sugars in cell-wall formation and in respiration, and of the nitrogen compounds in the synthesis and the break-down of protein. Oxidizing systems are also included, but only such as are known to be characteristic of the above-mentioned group of plants. There are copious bibliographies and an index.

MICROBES AND ULTRAMICROBES. *An Account of Bacteria, Viruses and the Bacteriophage. The Dial Press Monographs on Biological Subjects.*

By A. D. Gardner (with an Appendix by G. R. de Beer). The Dial Press, New York. $1.50. 4 x $6\frac{1}{2}$; viii + 120; 1930.

A good account of the bacteriophage, with preliminary chapters on bacteria and viruses, written for biologists in general. It can be recommended to intelligent lay readers as well as to biologists. There are brief but well-selected literature lists for those desirous of pursuing the subject.

BOTANISCHE VERSUCHE OHNE APPARATE. *Ein Experimentierbuch für jeden Pflanzenfreund.*

By Hans Molisch. Gustav Fischer, Jena. 9 marks (paper); 10 marks (cloth). $6\frac{1}{2}$ x $9\frac{1}{2}$; xii + 200; 1931.

A demonstration of what can be done by a really ingenious mind. The author shows how a wide variety of botanical demonstrations and experiments can be made without apparatus or with such apparatus as a sewing needle or a match; and the experiments are good ones.

AN INTRODUCTION TO PLANT PHYSIOLOGY.

By W. O. James. Oxford University Press, New York. $2.50. $4\frac{3}{4}$ x $7\frac{1}{4}$; viii + 259; 1931.

An elementary text in plant physiology, clearly written and with a fairly extensive set of experiments involving little apparatus. The book contains no references, either to original papers or to other textbooks.

MORPHOLOGY

OUTLINE OF COMPARATIVE EMBRYOLOGY. *By Aute Richards. John Wiley and Sons, New York.* $5.00. 5¾ x 9; 444; 1931.

This book aims to follow the same method so successfully employed in the teaching of anatomy: namely, to introduce the student first to the principles of general comparative embryology. The book is divided into two parts. Part I takes up those phases of development which are passed through in the embryology of most higher forms of animals. In selecting the topics for discussion the author has followed the single criterion of showing "only how the single-celled fertilized egg arrives at the multicellular conditions characteristic of the fully formed, but undifferentiated young organism in which the organ systems are established." No attempt is made to follow through all details or to repeat descriptions unnecessarily. With this general development of the subject the student can easily proceed to detailed work. Part II considers such miscellaneous problems as the origin and development of germ cells, the germ-layer and the recapitulation theory, asexual reproduction, parthenogenesis and the determination problem. The book is well illustrated, contains charts and tables of classification, a glossary, an extensive bibliography and an author and subject index.

SIMPLE LESSONS IN HUMAN ANATOMY. *By B. C. H. Harvey. American Medical Association, Chicago.* $2.00. 5¼ x 8¼; 434; 1931.

The man in the street probably retains a little of what was taught him in school about anatomy and physiology. But these vestiges are meager and often confused. To add to the confusion, he has probably read various articles, not always too authentic, on such fashionable subjects as the endocrine glands. To remedy this condition the Editors of *Hygeia* requested Professor Harvey to prepare a series of articles which would make the truths of anatomy generally available and intelligible. These articles form the present book. It is not painless science for the lazy minded. However skilfully they may be presented, no man can hope to understand such complex subjects as anatomy and physiology by a process of mental osmosis. But if the reader is willing to do his reasonable share of work, he will find this a fascinating account of the structure and development of the body and of the way in which it works.

A TEXT-BOOK OF NEURO-ANATOMY. *By Albert Kuntz. Lea and Febiger, Philadelphia.* $5.50 net. 5⅞ x 9¼; 359; 1931.

This excellent text-book by the author of *The Autonomic Nervous System*, reviewed in Volume V, correlates the anatomical details of the human nervous system with the fundamental structural plan of the vertebrate nervous system and discusses the anatomical structure of its parts in the light of our present knowledge of developmental and physiological relationships. There are bibliographies for each chapter and an index. We found the following passage especially interesting:

> The associational cortex, doubtless, must be regarded as of paramount importance in all the higher cortical functions. It must not, however, be regarded as the seat of intelligence or even of any particular mental or psychic process. Every mental or psychic process probably must be regarded as a function of the entire cortex. Its normal performance, furthermore, depends on the integrity of the cortex as a whole. That the cortex is not equipotential with regard to its higher psychic functions, however, is amply demonstrated by the defects resulting from lesions or loss of tissue in various cortical areas.

SOMATICAL INVESTIGATION OF THE JAVANESE 1929. *By D. J. H. Nyèssen. Anthropological Laboratory of Java, Bandoeng.* 6⅛ x 9½; x + 119 + 42 plates + 3 pp. appendices; no date.

This book represents only a start in the study of the anthropology of the Javanese; as such it is lacking in many of the anthropological measurements we should like to see, and more particularly in an adequate statistical treatment of the material.

The investigation was carried on from a

geographical standpoint because of the fact that considerable geographical isolation exists, and this seems to be the principal factor in accounting for the marked variations among people inhabiting different localities of Java.

The photographs of the natives are not highly significant from a somatological viewpoint as the people photographed were dressed in various costumes and no attempt was made to do the photographing against a scale. When the photographic materials gave out the workers resorted to making sketches, which convey only vague and general ideas.

The most useful parts of the book are in Dutch. A brief introduction, abstract of the results and conclusions are in English.

A Description of the Planes of Fascia of the Human Body. *With Special Reference to the Fascia of the Abdomen, Pelvis and Perineum.*

By B. B. Gallaudet. Columbia University Press, New York. $2.00. 5⅜ x 8; 75; 1931.

From his dissection of thirty-four cadavers Dr. Gallaudet feels that the descriptions of the fascial planes in the standard English and American textbooks are inadequate, especially for the abdominal, pelvic and perineal regions, and fail to make clear the continuity of fascial planes. His book is therefore intended to give a more adequate description.

Grundriss der Entwicklung des Menschen.

By Alfred Fischel. Julius Springer, Vienna and Berlin. 11 marks (paper); 12.60 marks (cloth). 6¾ x 10¼; vi + 141; 1931.

A shorter treatment of the material in the author's *Lehrbuch der Entwicklung des Menschen*, which was reviewed in Vol. IV of this Review. It summarizes the minimum data on human embryology which a medical man should know. There is an index but no bibliography.

PHYSIOLOGY AND PATHOLOGY

Physiologie Normale et Pathologique des Reins.

By L. Ambard. Masson et Cie, Paris. 55 francs. 6½ x 10; 502; 1931 (paper).

As Legueu says in the preface to this book, there are authors who need no introduction. Ambard's work on the functional relation between the concentration of a substance in the blood and in the urine and the rate of excretion of that substance, as well as on other phases of renal physiology and pathology, is well known. One of the most interesting parts of the book is that in which the derivation of this relation from experimental data is described. The experience of the nineteen years since the formula was proposed convinces him that within a wide range of conditions it describes the facts accurately but that for rates of excretion of urea of more than 100 grams in 24 hours the ureosecretory constant is higher than under normal conditions. For the explanation of renal thresholds he develops an ingenious theory of renal secretion, according to which, for instance, different intracellular proteins combine with and concentrate HCl and NaOH from the blood. These latter finally combine to form NaCl, which is excreted into the uriniferous tubules. The renal threshold is defined as the position of the isoelectric point of the intracellular proteins at which the rate of excretion of chlorine becomes a minimum. This theory has the advantage of explaining the concentration of excreted substances without recourse to any mystical "vital force" but, as a result of the inevitable technical difficulties of the study of intracellular physiology, it is less firmly founded on an experimental basis than is the author's work on the gross physiology of the kidney.

As to renal pathology, Ambard accepts Widal's division of nephritis into hydremic and azotemic. The former is primarily a derangement of the excretion of NaCl and can be best treated by putting the patient on a salt-free diet. There is much else in the book which will interest both the physiologist and the clinician. We cannot but admire the clarity with which the author treats a complex subject

as well as the candor with which he admits the limits of our present knowledge.

YELLOW FEVER. *An Epidemiological and Historical Study of Its Place of Origin.*

By Henry R. Carter. Edited by Laura A. Carter and Wade H. Frost. The Williams and Wilkins Co., Baltimore. $5.00. 6 x 9; xii + 308; 1931.

This is a contribution of first rate importance to the natural history of disease. Dr. Carter devoted his life largely to the study of yellow fever in the field and in the library. There is a kind of first-hand authoritativeness about his knowledge and opinions which will make this volume stand as a classic in its field. The editing and seeing of the book through the press was a labor of love by his daughter and his devoted friend, Dr. W. H. Frost. Dr. Carter would have been pleased with the careful and sympathetic handling of his material.

The first section of the book deals with the epidemiology of yellow fever, and is the best brief account known to us of this complicated subject. This is followed by a discussion, of about equal length, of diseases which have, or might have, been confused with yellow fever in the past. The remaining two-thirds of the book is concerned with the place of origin of yellow fever, and reaches the conclusion by a superb piece of sustained reasoning and critical weighing of evidence, that Africa (and particularly West Africa) must be accorded the dubious distinction of having been the place of origin of this dreadful disease.

There is an extensive bibliography and adequate author and subject indexes.

PHANTASTICA. *Narcotic and Stimulating Drugs.*

By Lewis Lewin. E. P. Dutton and Co., New York. $3.75. 5½ x 8½; xi + 335; 1931.

In this book the first investigator of the pharmacology of *Anhalonium lewinii* and kava-kava brings together a large mass of interesting information, pharmacological and anthropological, on euphorica (opium and its derivatives, and cocain), phantastica (*Anhalonium lewinii*, Indian hemp, *Agaricus muscarius*, Solanaceae, *Banisteria caapi*, *Gelsemium sempervirens*, and the loco herbs), inebriantia (alcohol, chloroform, ether, benzine, and nitrous oxide), hypnotica (chloral, veronal, paraldehyde, sulphonal, potassium bromide, kava-kava, and kanna), and excitantia (camphor, betel, kat, coffee, tea, kola, maté, *Ilex cassine*, pasta guarana, cocoa, tobacco, parica, arsenic, and mercury). An interesting aspect is the use of many of these substances in religious rites, especially the use of the phantastica to produce visions. In his discussion of the vexed question of the hereditary effect of alcohol Professor Lewin seems to us rather uncritical. Tabulations are given of the alcoholic relatives, direct and collateral, of alcoholic or mentally subnormal persons, but we look in vain for corresponding tabulations of the alcoholic relatives of normal controls. Furthermore, if the statistical association of alcoholism in the parent and alcoholism or mental defect in the offspring were proven, we should still have before us the important question: was the parental alcoholism the cause of the defect in the offspring, or were both the outward and visible signs of a hereditary neural instability? It is strange that Professor Lewin, who in other parts of his book emphasizes the importance of constitution, should be blind to this possibility.

THE PHYSIOLOGY OF MUSCULAR EXERCISE. *By F. A. Bainbridge. Third Edition, rewritten by A. V. Bock and D. B. Dill. Longmans, Green and Co., New York.* $5.00. 5¾ x 8¾; viii + 272; 1931.

As a clear and thorough account of laboratory researches on the physiology of muscular exercise, this book ought to be welcomed by trainers of athletes, physicians, and all those connected with the health of the community. Although the book has in large part been rewritten, many of the conclusions originally reached by Professor Bainbridge remain unchanged. Throughout the book attention is drawn

to the differences in respiration, circulation and in the other systems of the body manifested by those who are trained and those who lack training in physical exercise. The benefits of exercise are mentioned, but no brief is held for more than ordinary amounts of exercise. It is a comfort to learn that "fortunately for most people the exercise consequent upon ordinary activities of life is sufficient to maintain the body in a state capable of responding adequately to usual effort."

PREVENTION OF PREMATURE SENILITY.
By Victor G. Vecki. The Stratford Co., Boston. $1.00. $4\frac{7}{8}$ x $7\frac{3}{8}$; 127; 1931.

Some sensible advice is to be found in this little book. The author, a practising physician, urges moderation rather than abstinence in the pleasurable things of life. He believes that the "energy of man, his courage, his enjoyment of work and of life, all, with hardly any exception, depend on his sexual power." As this power wanes, senescence waxes. The doctor argues that continued regular, and moderately frequent exercise of the sexual functions tends to postpone their waning, and that therefore the way to achieve serene and considerable longevity is to keep persistently at the good old *gymnastique d'amour*. Rabelais had the same idea. We are sceptical about its underlying biological assumptions. Furthermore there is the experimental observation that many an honest man has done his best to stave off old age by the plentiful and regular exhibition of so pleasant a medicament, only to find presently that he was powerless to get the prescription filled.

THE DOCTOR EXPLAINS.
By Ralph H. Major. Alfred A. Knopf, New York. $3.50. $5\frac{3}{4}$ x $8\frac{5}{8}$; xxiv + 277; 1931.

The history and science of modern medicine are interpreted in this book for the benefit of the layman. Many people devote a good deal of thought to their health, and acquire a conglomeration of vague and odd notions about the way their bodies work. To these persons a simple account of such matters as the rôle of the blood, heart disturbances and blood-pressure, vitamins, X-rays, antiseptics, anaesthesia, and many other items of physiology and medicine should be useful. The story of the history of medical practice is extremely interesting.

THE HEALTH OF THE SCHOOL CHILD. *A Study of Sickness, Physical Defects and Mortality. Public Health Bulletin No. 200. By Selwyn D. Collins, with an introduction by Taliaferro Clark. U. S. Government Printing Office, Washington.* 35 cents. $5\frac{3}{4}$ x $9\frac{1}{8}$ inches. x + 159; 1931 (paper).

A careful, thorough and sound study of morbidity, mortality, and physical defects in children. The data came from selected localities in Maryland, Missouri, and Florida, and cover a period of about ten years in the collecting. Some of the broad general results are: (1) that 57 per cent of the absences from school are due to sickness, and 43 per cent to other causes; (2) that the case rate for illness was 8 per cent higher in girls than in boys; (3) that 75 per cent of the children were found to have one or more physical defects other than defective teeth, vision or hearing; (4) that the six most important causes of death among children 5 to 19 years of age were accidents, tuberculosis, heart diseases, pneumonia, diphtheria, and appendicitis.

INTRODUCTORY STUDY ON VIOLIN VIBRATO. *Research Studies in Music, Department of Research of the Conservatory of Music, Number 1.*
By Louis Cheslock. Peabody Conservatory of Music, Baltimore. $1.00. $6\frac{3}{4}$ x $9\frac{3}{4}$; 79; 1931 (paper).

Most scientific research on music has been concerned with the physical analysis of the sound waves produced rather than with the physiological mechanism of their production. Ortmann's *The Physiological*

Mechanics of Piano Technique, reviewed in Volume V, was perhaps the first detailed analysis of a musical problem from the latter point of view. The present study from the same laboratory records the finger movements used in producing vibrato and the resulting variations in pitch, intensity, etc., and correlates the various factors involved. The speed of a normal vibrato is found to be about the physiological maximum for rhythmic movement.

DER EINFLUSS DER ERNÄHRUNG AUF DIE KONSTITUTION DES ORGANISMUS. *Ergebnisse experimentell-biologischer Forschungen.*

By A. Katase. Urban und Schwarzenberg, Berlin. 10 marks. 7 x 10; vi + 162; 1931 (paper).

A summary of the experimental work on animals done by Professor Katase and his students at the Pathological Institute of the Academy of Medicine at Osaka. It is in the nature of a preliminary report, and further work is in progress. Part A treats of the effects of inorganic salts on various systems of the body, on bacteria, and transplantable tumors. Part B deals with the effects of sugars, fats and albumens on bone, teeth, muscle and blood; and part C is concerned with the influence of alkalies, spices, radiation, etc. on the digestion.

THE EXCHANGE OF ENERGY BETWEEN MAN AND THE ENVIRONMENT.

By L. H. Newburgh and Margaret W. Johnston. Charles C. Thomas, Springfield, Ill. $2.00. 5½ x 8; xiv + 104; 1930.

This little book for the medical student on quantitative metabolism is more vivacious both in binding and in style than the run of textbooks. The account in the historical section of the execution of Lavoisier is new to us. According to this Lavoisier lost his head, not because he had been a Farmer General, but because Marât differed with him on the nature of fire.

ÜBER KLIMATOPHYSIOLOGIE.

By A. Loewy. Georg Thieme, Leipzig. 4.80 marks. 5¾ x 8½; 77; 1931 (paper).

A discussion of the effects of climate on human physiology.

BIOCHEMISTRY

DIE KATALYSE. *Die Rolle der Katalyse in der analytischen Chemie. II. Spezieller Teil, Zweite Abteilung: Biologische Katalysatoren. 2. Hälfte: Atmungsfermente.*

By Gertrud Woker. Ferdinand Enke, Stuttgart. 76 marks (paper); 79 marks (cloth). 6½ x 10; xix + 592; 1931.

The first part of this book, which devotes 200 carefully documented pages to the chemistry of the zymases, must give pain to all those who realize what a serious mistake God made in creating the genus *Saccharomyces*. The second part, dealing with the oxidation ferments, is not, we are glad to say, inconsistent with the spirit of the Eighteenth Amendment.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 365. Quantitative Stoffwechseluntersuchungen.* Containing following articles: *Die Methodik des pharmakologischen Arbeitsversuches*, by Otto Graf; *Ein neuer Universalapparat für die Mikroanalyse*, by Ernst Tschopp.

Urban und Schwarzenberg, Berlin. 7.50 marks. 7 x 10; 146; 1931; (paper).

This number of the Abderhalden handbook contains a discussion of methods of investigating physical and mental output, particularly with reference to the effects of drugs; and a description of a new apparatus for microanalysis and the technique of using it.

NUCLEIC ACIDS.

By P. A. Levene and Lawrence W. Bass. Chemical Catalog Co., New York. $4.50. 5⅞ x 9; 337; 1931.

The historical method of discussion used by the authors is well adapted to show where the emphasis has been in the past,

and towards what goal investigators should head in the future. The monograph is thorough, clear and well written.

SEX

MY FIGHT FOR BIRTH CONTROL.
By Margaret Sanger. Farrar and Rinehart, New York. $3.00. $5\frac{3}{4}$ x $8\frac{5}{8}$; vii + 360; 1931.

THE TRUTH ABOUT BIRTH CONTROL *with a Bibliography of Birth-Control Literature.*
By Norman E. Himes. The John Day Co., New York. 25 cents (35 cents in Canada). $5\frac{1}{4}$ x $7\frac{3}{4}$; 28; 1931 (paper).

THE CASE AGAINST BIRTH CONTROL.
By Edward R. Moore. The Century Co., New York. $2.50. 5 x $7\frac{1}{2}$; x + 311; 1931.

JUDGMENT ON BIRTH CONTROL.
By R. de Guchteneere. The Macmillan Co., New York. $2.00. $5\frac{1}{8}$ x $7\frac{3}{8}$; 224; 1931.

Of these four books two are for and two are against the birth control movement. Mrs. Sanger, the most widely known of the authors, gives an interesting account of her early life and her experiences as a nurse and social worker. These led her into a career as a crusader against the miseries which the bearing of too numerous offspring entail upon poverty-stricken women. That she had courage and determination to carry on this fight the book amply testifies. It is also made plain both in and between the lines of the book that she is an extremely shrewd person who has missed few bets. The picture is of a clever and disarming marshal, who gained adherents to her cause by the force and charm of her personality as much as by the principles involved. There are gaps in her account that arouse the reader's curiosity. But the story is a fascinating one.

Mr. Himes' title is slightly misleading. One opens his little book expecting to find an array of figures backing up definite statements concerning the effect of birth control upon the individual or upon the general population. Instead he discusses the status of the birth control movement in European countries and its inception and development in America. He finds politicians particularly blind to any suggestions for alleviating the miserable state of that class of people which bears the brunt of populating the world while theoretically the least fitted to do so. Those who are most anxious to keep the birth rate high are the very ones who refuse to carry their share of the burden. In France, a committee of twenty-five leading a "large-families-for-other-couples" movement had between them nineteen children. There is much in this little book concerning the legal aspects of the birth control movement, and the attitude of the American Medical Association, as well as individual physicians, economists, sociologists and the Catholic clergy towards it. The author believes that within thirty years methods of birth control will be simplified and made certain and will become common knowledge. Federal laws of today will fall completely into disuse and by 1965 we shall have a stationary population "pegged" at 160,000,000. Perhaps.

The two opposing treatises on birth control are contributed by Catholics. The author of *The Case Against Birth Control* is chairman of the Committee on Population Decline and Related Problems of the National Conference of Catholic Charities Investigation, which was appointed to make an exhaustive study to ascertain the medical, social, and economic results of artificial birth prevention. The official results of this investigation are issued in this book. Cardinal Hayes contributes a brief introduction. We find little in its pages of the kind of sympathy for suffering women that so excites Margaret Sanger's emotions. No phase of the birth control movement finds favor with the Commission. All evidence, they find, points to alarming consequences to the individual and the state if the use of contraceptives becomes general. There is a denial of the generally accepted idea that the Catholic Church requires the raising of as large families as possible. "The Catholic Church believes in birth-control but by self-control." Continence alone finds favor with the Church. Such topics as overpopulation of the world, food supply and the law of diminishing returns are

treated with skilful dialectic, but not quite convincingly. The moral wrong of contraception, the law and its enforcement, and the two-child system are discussed.

The last of these books is an expansion of the original French version. As in the previous book, the author handles his topics deftly, whether they be economics that he discusses, eugenics, the medical aspect of birth control or morals. His conclusion in each case is that "science and common sense are agreed in proclaiming the need of respect for the natural law which orders human action even in its more remote manifestations." In the final chapter he gives the Catholic view of birth control. Biologists are not likely to find the arguments convincing.

Looking at these four books in a detached manner, they form a perfect example of a perennially entertaining phenomenon of human biology. Two groups of human beings, having before them one and the same set of objective data, reach absolutely different sets of conclusions, emotions and motivations. The protagonists of each view think that they, and they alone, are right. They entertain no doubt that their opponents are immoral, illogical, stupid, unpatriotic, irreligious, unscientific—in short perverse jackasses. Yet all belong to the species *Homo sapiens*. All have only the same identical set of facts to work on. Neither side knows anything that the other doesn't. Man is assuredly a remarkable mammal.

DIE BESCHNEIDUNG BEI MANN UND WEIB. *Ihre Geschichte, Psychologie und Ethnologie. By Felix Bryk. Gustav Feller, Neubrandenburg.* 15.60 marks (paper); 18 marks (bound). $6\frac{1}{4}$ x $9\frac{1}{4}$; x + 319; 1931.

That this thorough and scholarly study of circumcision, subincision and the like sexual mutilations has had a wider popularity than its author had anticipated is evident from the fact that he has found it advisable to supplement the book with a glossary in which the Latin and other technical terms are translated into a tongue understanded of the people. Bryk rejects the explanations of the origin of circumcision on hygienic grounds which have been current since the days of Philo and bases the practice on a foreskin complex due to the desire of the male child to have a denuded glans, which is to it the emblem of virility, and on a supposed retardation of ejaculation, which makes the circumcised man a more satisfactory partner in coitus.

An interesting section is devoted to the *Sanctissimum Præputium Christi*. This was carefully preserved by the Mother of God, who before her Assumption gave it, according to one account, to St. John; according to another, to the Magdalen. In the course of ages the precious relic was lost; but it was ultimately brought by angels to Charlemagne, who presented it to the Pope. During the sack of Rome in 1527 it was stolen by an Imperialist soldier, but was fortunately recovered. When the reliquary was opened, the celestial odor which was diffused attested its authenticity to even the most sceptical. Besides this relic, with its unimpeachable pedigree, no fewer than eleven churches boast *Præputia Christi* among their treasures.

A THOUSAND MARRIAGES. *A Medical Study of Sex Adjustment. By Robert L. Dickinson and Lura Beam. Foreword by Havelock Ellis. The Williams & Wilkins Co., Baltimore.* $5.00. 6 x 9; xxv + 482; 1931.

This interesting and valuable product of the National Committee on Maternal Health is based upon case records from the practice of Dr. Dickinson extending over many years. In subject matter it touches upon, with varying degrees of thoroughness and penetration, almost every aspect of sex physiology and psychology. The material came from "the educated, urban couple, socially normal, the wife in good general health." Some of the biologically more interesting conclusions are:

The woman has a capacity for sexual desire, lifelong, inconsistent and fluctuating, and found in every individual fully studied.

Fertility has an important psycho-sexual correlation with coitus.

Even where there are no children, complete unity in marriage depends on sexual unity.

Sexual abstinence in marriage is ordinarily impracticable.

While intimate sex relations are most acceptable to women in terms of the total personality, there is a definite physical technique kept perfect only through experiment.

The sexual difficulties are infrequently organic in the woman and save in exceptional cases, not functional.

Because of the original material presented and the careful, critical treatment given it, this volume constitutes a valuable supplement to the studies of Katherine Bement Davis and G. V. Hamilton. It will be found a valuable reference work by all students of the physiology of reproduction. There is an index, but no bibliography.

PROCEEDINGS OF THE SECOND INTERNATIONAL CONGRESS FOR SEX RESEARCH, LONDON, 1930.

Edited by A. W. Greenwood. Oliver and Boyd, London and Edinburgh. 21 shillings net. 5½ x 8¾; xi + 637; 1931.

A well-edited and printed collection of somewhat too brief and scrappy papers on recent results and opinions in the field of sex and reproduction. We gather that the necessity of printing the outpourings of persons who had been invited to attend the Congress, and in good faith did so, is the real reason for the inclusion of a few dubious essays, which, in fact, only the equivalent of senatorial courtesy could distinguish from quackery.

THE CONQUEST OF OLD AGE. *Methods to Effect Rejuvenation and to Increase Functional Activity.*

By Peter Schmidt, translated by Eden and Cedar Paul. E. P. Dutton and Co., New York. $5.00. 6 x 9¼; xvii + 313 + 40 plates; 1931.

Just another one of those books about rejuvenation, *à la mode* Steinach. It has more pages and pictures than the general run of such treatises, but is not more convincing. It is louder but not funnier than some.

Reginald the Office Boy, who has a curious, enquiring mind, read all the case histories in the book, which probably no one else will ever do. His comment was that all the testimony as to the degree of rejuvenation effected seemed to come from Papa, and that he should like to know what Mama's views were on the point.

BIOMETRY

SOMMARIO DI STATISTICA.

By Giorgio Mortara. Università Bocconi di Milano, Milan. 40 lire. 7 x 10; xv + 393; 1931 (paper).

The intention of the author is to give to his students in this summary a simple and facile introduction to statistical methods. In fact, in it we find only a partial exposition of the elements of methodology and of some of its applications to economics. But it has a noteworthy and characteristic feature in that the author emphasizes the logical basis in statistical applications. It is essential to train the student in this way, since in order to avoid tedious mental labor he quite often prefers to apply the complex and beautiful formula although he does not understand it or its significance.

If this is the great merit of this book, it is also its greatest fault. Written in a rather difficult style, prolix, it also lacks any explanation for the calculation of the necessary and most useful constants. This, together with the noticeable deficiency in illustrative examples, would tend to render the subject obscure to the student, we feel.

The division of the book is the usual: Observation, description, and interpretation of phenomena. The second part is especially well done and Professor Mortara's observations on correlations are instructive and incisive.

An ingenious idea of the author has been to oblige the student to use the *Annuario Statistico Italiano* as a source for all exercises. In this manner the student not only learns the practical application of theory but he also learns to consider and to evaluate the important factors in the progress of his country.

LIFE EXPECTANCY OF PHYSICAL PROPERTY. *Based on Mortality Laws.*

By Edwin B. Kurtz. The Ronald Press Co., New York. $6.00. $5\frac{3}{4}$ x $8\frac{1}{2}$; xiv + 205; 1930.

An attempt to apply actuarial methods to physical property, with a view to a more accurate determination of depreciation. The subject is obviously an important one to the engineer and accountant. We had hoped to find that the author, approaching the subject from a new direction, would have offered some new ideas which might be useful to biological students of duration of life. To our disappointment, we have found little of value to us in the book; the ideas presented, in so far as they are novel, seem to us of small importance.

PROBABILITÉS ET STATISTIQUES.

By R. de Montessus de Ballore. Librairie Scientifique Hermann et Cie, Paris. 60 francs. $6\frac{1}{2}$ x 10; ix + 211; 1931 (paper).

An attempt to make the binomial distribution the basis of frequency distributions. The author displays considerable mathematical ingenuity, but we do not believe that statisticians and biometricians will find his work of practical importance.

MEASUREMENTS OF BIRDS. *Scientific Publications of the Cleveland Museum of Natural History, Vol. II.*

By S. Prentiss Baldwin, Harry C. Oberholser, and Leonard G. Worley. Illustrations by James M. Valentine. Cleveland Museum of Natural History, Cleveland. $1.00 (paper); $1.65 (cloth); postage 20 cents extra. $6\frac{3}{8}$ x $9\frac{3}{8}$; ix + 165; 1931.

A valuable handbook for anyone who has occasion to measure birds. It gives a clear description of the methods of taking all the measurements which have been found useful in ornithology, and should assist materially in bringing about a reasonable standardization of techniques.

PSYCHOLOGY AND BEHAVIOR

CHANGE OF INTERESTS WITH AGE. *Based On Examination of More Than Two Thousand Men Between the Ages of Twenty and Sixty Representing Eight Occupations.*

By Edward K. Strong, Jr. Stanford University Press, Stanford University, Calif. $4.00. $5\frac{7}{8}$ x $8\frac{3}{4}$. xix + 235; 1931.

A statistical study of the interests of men of from 20 to 60 years of age, particularly in the changes which take place with advancing age. It is based on data regarding the likes, dislikes, and attitudes of individuals towards such items as occupation, amusements, activities and traits. The author finds strong support for the hypothesis that interests are responsible for choice of occupation rather than a resultant of it. Older men seem to be no more catholic in their interests than younger men. Men between 20 and 60 are in surprising agreement regarding the peculiarities of other people and their own characteristics. The greatest change in interest (50 per cent) occurs between the years 25 and 35, 20 per cent between 35 and 45, and 30 per cent between 45 and 55, with little change from 55 to 65. In general, changes in interest are not great with increased age except in the dislike for activities involving physical skill and daring and for changes or interference with established habits or customs. Things which are liked most at 25 years of age are liked better and better as age advances. The body of the book contains many tables and figures, while in the appendix are the basic tables dealing with 420 items from the Vocational Interest Blank. There is a brief bibliography and an index. Professor Walter R. Miles contributes a foreword.

A FIELD STUDY OF THE CHIMPANZEE. *Observations of Chimpanzee Behavior and Environment in Western French Guinea. Comparative Psychology Monographs, Vol. 8, No. 1, Serial No. 36.*

By Henry W. Nissen (with a Foreword by Robert M. Yerkes). The Johns Hopkins Press, Baltimore. $2.00. $8\frac{3}{4}$ x 10; vi + 122; 1931 (paper).

The study of which this monograph is a report was undertaken primarily to determine the feasibility of field studies to discover methodology and technique for observations of chimpanzees in their native habitat. Dr. Nissen spent nearly three months in the bush of French Guinea observing the social organization, activities, nesting and feeding habits, and social behavior of these animals. From these preliminary studies he feels that "future field work may well be expected to reveal many instances of so-called intelligent behavior. But it is nevertheless my opinion that field work will not disclose the upper limits of chimpanzee intelligence; that the ultimate capacities of these apes for complex behavior will be found in our laboratory experimental situations." Nevertheless, a precise knowledge of the native environment of the chimpanzee may be useful to the student of comparative psychobiology, for modes of behavior which, in the laboratory, appear to be so-called higher mental processes, may, in the light of previous experiences in the bush, be due to the operation of simple memory or transfer of training mechanisms.

THE MENTAL DEFECTIVE. *A Problem in Social Inefficiency.*

By Richard J. A. Berry and R. G. Gordon. Whittlesey House, McGraw-Hill Book Co., New York. $2.50. 5½ x 8; xiii + 225; 1931.

The theme-song of this somewhat gloomy treatise is that "all available evidence goes to suggest that, in any average population . . . . the individual passes on into the stress of civilized life with a brain imperfectly adapted to combating those stresses and remains at the level of dull normality or even feeblemindedness," because the last layer of the cortex fails to develop properly with the most of us. It is urged that it is "the high grades and dull normals who appear to be the real issue and from whom the potential 'menace' arises, for they are sufficiently developed to possess strong animal propensities without a sufficient power of control over such passions either in their own interests or those of others."

We should like to be shown more definitely how one is trained to estimate the number of normally developed brain cells in view of the allegation that "degree of mental normality attained by any individual is in strict accord with the state of development and numbers of brain cells possessed."

THE JUNGLE OF THE MIND.

By Edgar J. Swift. Charles Scribner's Sons, New York. $2.50. 5½ x 8; x + 340; 1931.

A pungent criticism of the way in which we use our minds. The extreme gullibility of man, his desire and willingness to believe in the mysterious irrespective of how ridiculous it may be, his weakness for vague and empty phrases that sound well, are again paraded before us in all their naked obsceneness. The chapters dealing with psychoanalysis are to the point, and the author tries to be rather more fair to Freud than many present day writers are. To quote: ". . . . we may say that Freud performed a great service by forcing the attention of psychologists and psychiatrists upon the subconscious and unconscious processes. He has greatly clarified the dynamic view of mind, and he started Jung, Adler and others upon independent and valuable lines of investigation." This is surely not an overstatement of the significance of a great man.

THE MAJOR FORMS OF INHIBITION IN MAN.

By Ernest B. Skaggs. The University of Chicago Press, Chicago. $1.50. 6 x 9; vii + 104; 1931 (paper).

This excellent little book brings together the chief experimental work on the various forms of inhibition which the student encounters in his study of psychology. In the final chapter McDougall's drainage theory of the physiological basis of inhibition is discussed. The working hypothesis of the author is that the explanations of mental phenomena must be sought in terms of physiological processes. How-

ever, he does not throw out consciousness with the bath water but uses it as a sign of the underlying neural processes.

PSYCHOLOGY: *Science or Superstition?* *By Grace Adams. Covici, Friede, New York.* $2.50. 5 x 8¼; 299; 1931.

In the title of this book the author has raised a provocative and pertinent question. The material on which an answer depends is presented in a vigorous, penetrating survey of the development of modern psychological thought from Wundt, through James, Titchener, Hall, Freud, Jung, Watson, and McDougall, to the *Gestaltists*, Koffka and Köhler. One chapter is devoted to mental hygiene and mental testing. The author writes with a light touch which makes the book amusing as well as informative reading.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

THE NUTTALL ENCYCLOPÆDIA. *Dealing with Biography, Geography, History, Mythology, Literature, Science and Invention, Religions of the World, Politics, Art, Music, the Stage, Sport, Business, Law, Etc. Also Appendices giving Populations, Pseudonyms of Writers and Artists, Characters in Drama and Fiction, Etc.*

Edited by G. Elgie Christ and A. L. Haydon. Based on the Original Work by James Wood. Frederick Warne and Co., New York. $3.00 net. 5½ x 8¼; vii + 695; 1930.

This book offers a large amount of information in small compass "to the careful Newspaper Reader, to Heads of Families, with children at school, whose persistent questions have often to go without an answer, to the Schoolmaster and Tutor, to the Student, to the Busy Man and Man of Business." The editors' statement of the principles on which the revision was carried out is interesting, especially in a book designed primarily for British consumption.

In Science and Invention the amazing progress of the last few years, which have seen the invention and development of the petrol engine, mechanical flight, and wireless telegraphy and telephony, makes large additions necessary. In Geography the Great War altered the face of Europe and affected other quarters of the globe. Biography, History, Literature and Art are all subjects that call for continual expansion. By careful selection and arrangement the Editors trust that they have done justice to all these new claims without unduly curtailing the space given to the older subjects, such as Mythology, Classics, Religion, and Philosophy, or sacrificing any of the features that have won so much appreciation for the Encyclopædia in the past. If any discerning reader objects that the new Encyclopædia is now less classical and more scientific than the old one, he will surely admit that that is but in accordance with the trend of modern life and unavoidable.

THE SCIENTIFIC OUTLOOK. *By Bertrand Russell. W. W. Norton and Co., New York.* $3.00. 5¾ x 8¼; 277; 1931.

Bertrand Russell has such a gift for plausible and striking statement that one readily overlooks defects which would be extremely annoying in a less brilliant writer. In the present book he considers the nature of science, and what the tendencies which he sees in science would lead to, if unchecked, in the way of a society. The picture, we may say, is not attractive; but we confess to an absence of alarm. For one thing, as Russell himself points out, there are other tendencies in human nature, and it is highly improbable that the scientists will ever be allowed to organize and run their own kind of a world. For another thing, we have a feeling that nothing that we personally can do about it is likely to have much effect on the evolution of society; and we are not so constituted as to lose too much sleep over a future which we cannot influence.

The volume is well worth looking into; it abounds in quotable passages, many of which, of course, use the Shavian technique of "stating the obvious in terms of the scandalous," but which are not less amusing because the trick is old.

THE DEGRADATION OF SCIENCE. *By T. Swann Harding. Farrar and Rinehart, New York.* $3.00. 5¾ x 8½; xi + 386; 1931.

This book is by the author of *Fads, Frauds, and Physicians*, which was reviewed here in Volume VI, Number 3. The title of the present volume seems to us a little misleading. Actually it deals with everything in our present civilization to which the author objects—at least we hope so. Certainly if such diverse matters as the size of Mr. Eugene G. Grace's bonus and the dishonesty of the Christus at Oberammergau (the latter incident inaccurately told) are included in a book ostensibly devoted to science, little can have been omitted. There is nothing particularly novel in his complaints; they have been voiced before by such diverse figures as Brooks Adams, Thorstein Veblen, Stuart Chase, and Herbert Hoover, and, to us at least, more effectively. The documentation is extensive, but not always impressive. The language is sometimes careless, to say the least. For example: "Has law then nothing to do with ethical theory? Stammler would hold quite the contrary." This, we assume, means that Stammler holds that the law and ethical theory are connected. But then we read: "'Must not the content of just law be derived from ethical theory?' he [Stammler] asks naively and he replies: 'We deny this absolutely.'"

A Cartoonist's Philosophy.

By Percy Crosby. Percy Crosby, McLean, Va. $2.00. 5¼ x 7¾; 252; 1931.

The creator of "Skippy" has got so "het up" over prohibition and various other subjects that he is publishing a book about them at his own expense. The first part is a narrative of a summer in Normandy and England; the second is made up of miscellaneous essays and dialogs on prohibition, religion, atheism, art, child psychology, government, or what have you.

Indians, Crocodiles and Monkeys.

By John V. Deuel. The Century Co., New York. $2.00. 5 x 7½; viii + 247; 1931.

A rollicking story of the experiences of a young flying officer of the United States Army air corps who, temporarily relieved from duty, sailed as Spanish interpreter and wireless operator on the yacht Samona, on a cruise from California, through the Canal, along the northern coast of South America and several hundred miles up the Orinoco. Being one of those lucky individuals to whom things happen, and possessing the gift of writing entertainingly, his book will find many interested readers, especially among adventure loving youth.

A Short History of Atomism *from Democritus to Bohr.*

By Joshua C. Gregory. A. and C. Black, London; The Macmillan Co., New York. 10s. 6d. net (England); $3.50 (U. S. A.). 5½ x 8½; 258; 1931.

A good account of the endeavors of the human mind to reduce the world to a collection of little black balls. It leaves the non-physicist with the old question still on his hands as to how far the little black balls really correspond to something in the external world, and how far they merely represent the only way the human mind can think of the external world. The story is a fascinating one, and so far as we can judge, accurately and judiciously told in the present book.